SCIENCE

FRIDAY, MARCH 31, 1911

CONTENTS

The Beginnings of Intelligence: PROFESSOR S. J. HOLMES 473

The Galton Chair of Eugenics 480

Scientific Notes and News 481

University and Educational News 485

Discussion and Correspondence:—

The Air we breathe: WILLIAM KENT, WILLIAM J. CROWELL, JR., LOUIS CLEVELAND JONES. *University Fellowships:* PROFESSOR GUIDO H. MARX, DR. C. V. BURKE.. 486

Scientific Books:—

Kiebel and Mall's Human Embryology: PROFESSOR H. MCE. KNOWER 493

Special Articles:—

The Origin of Nine Wing Mutations in Drosophila: PROFESSOR T. H. MORGAN ... 496

The National Conference Committee on Standards of Colleges and Secondary Schools: PROFESSOR FREDERICK C. FERRY.. 499

Anthropology at the Providence Meeting: DR. GEORGE GRANT MACCURDY 500

The Fifth Annual Meeting of the Entomological Society of America: PROFESSOR ALEXANDER D. MACGILLIVRAY 507

Societies and Academies:—

Section of Biology of the New York Academy of Sciences: L. HUSSAKOF. *The New York Section of the American Chemical Society:* C. M. JOYCE. *The Helmintholog-ical Society of Washington:* MAURICE C. HALL 507

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE BEGINNINGS OF INTELLIGENCE[1]

NOTHING shews more the force of habit in reconciling us to any phenomenon, than this, that men are not astonish'd at the operations of their own reason, at the same time, that they admire the *instinct* of animals, and find a difficulty in explaining it, merely because it can not be reduc'd to the very same principles. To consider the matter aright, reason is nothing but a wonderful and unintelligible instinct in our souls, which carries us along a train of ideas, and endows them with particular qualities, according to their particular situations and relations.—David Hume, "Treatise on Human Nature."

We all have a certain curiosity regarding the evolutionary history of our various powers and attributes, but from many points of view an unusual interest attaches to the first development of intelligence. The word intelligence is used in a variety of senses by writers on comparative psychology and any discussion of the origin of intelligence would be fruitless unless the meaning in which the term is employed be understood. One of the foremost of comparative psychologists, the acute Father Wasmann, defines intelligence as "the power of conceiving the relation of concepts to one another and of drawing conclusions therefrom. It involves abstraction, deliberation and self-conscious activity." Intelligence, according to Wasmann, is the God-given attribute of man alone; its possession separates man from brute by an impassable barrier.

Many comparative psychologists, among whom we may mention Lloyd Morgan, Forel and Loeb, adopt as a criterion of intelligence the power of forming asso-

[1] Read before the meeting of the Sigma Xi of the University of California, December 7, 1910.

ciations, or associative memory, and we shall follow the usage of these writers. It is obvious that the possession of this faculty marks an important step in advance upon the creatures whose actions are fatally determined by their instinctive make-up. From its beginning in forms in which the simplest associations are established only after a large number of experiences, intelligence has assumed a rôle of ever-increasing importance in the evolution of animal life, until in man, who is notoriously a weakling compared with the large beasts with which he has had to contend it became the main factor to which the human species owes its supremacy in the struggle for existence.

In considering the origin of intelligence one is naturally led to the subject of the relation of intelligence to instinct. Formerly it was the custom to contrast these two faculties as if they represented diametrically opposed types of activity. Instinct was regarded as something unalterably fixed, machine-like and practically perfect in its adaptation to the needs of the animal; intelligence was recognized as the anthithesis of all these qualities—variable, plastic and eminently fallible. With the establishment of the theory of evolution writers became more disposed to discover the kinship and filiation of instinct and intelligence and they have given us a variety of views as to the relation of these faculties.

Basing his theory on Lamarck's doctrine that instinct is inherited habit, G. H. Lewes attempted to explain instinct as "lapsed intelligence." Performances which are learned with difficulty come, after sufficient repetition, to be carried out automatically and without any intelligent guidance. If the acquired facility of performing these acts is inherited and the acts are repeated generation after generation, it is probable that they might finally be performed by an individual without any previous instruction at all; that is, they would become instinctive. An animal's instincts, according to this view, represent the stereotyped and mechanized behavior which its ancestors found to be profitable; their adaptiveness rests upon the wisdom acquired by ancestral experience. More recently this view has been upheld by Eimer, and in a less extreme form by Romanes, Wundt and many others.

One difficulty with the theory of lapsed intelligence is that it involves the acceptance of the doctrine of the transmission of acquired characters, which has come to be a very questionable biological theory. But another and more fundamental difficulty is revealed by recent work on the behavior of lower organisms. If instinct were derived from intelligence by a sort of mechanizing process we should expect, as Whitman has urged in his criticism of Lewes's theory, to find intelligence dominant in lower forms of life, and that acts which are instinctive in the higher animals would be intelligently performed by the lower ones. The work that has been done on the behavior of lower organisms enables us to state with confidence that such is not the case. In several large phyla of the lower invertebrates there has not, as yet, been demonstrated the least glimmer of intelligence; and, as we pass up the scale of life, intelligence gradually supersedes instinct, not the reverse. We can say with some degree of assurance that, however the transition may have been effected, intelligence has grown out of purely instinctive behavior.

It is not possible, however, to fix, except with the rudest approximation, the stage of evolution at which intelligence makes its first appearance. The transition from instinct to intelligence has been made, in all probability, not once, merely, but several

times along different lines of descent. Intelligence in the vertebrates doubtless arose independently from that of the insects, and the intelligence exhibited here and there among the mollusks probably arose independently along a third line of development. Intelligence makes its appearance at a certain stage of organization along whatever line such a stage may have been reached.

Up to the point at which the power of associative memory becomes manifest there has been progress along many lines which has prepared the way for the evolution of this new faculty. Behavior has not only become more complex, but it has become more plastic and capable of easy modification to suit new conditions. The lower organisms do not always react in a particular way to a given stimulus. What reaction occurs may depend upon the number of previous stimulations, the supply of food, exposure to different environing conditions, and numerous other factors which influence the internal state of the organism. The behavior of many lower animals is plastic and adaptive to a remarkable degree, and to a superficial consideration often gives the appearance of a considerable degree of intelligence, without there being any detectable power of associative memory. This plastic and varied behavior not only simulates intelligence, but it secures for the organisms many of the advantages which intelligence confers. It adapts the animal to a more varied environment, and gives it the power of meeting a given situation in more than one way, so if one kind of response does not suit, another may be more successful. Let us glance briefly at some of the ways in which behavior may be modified.

A very general change of behavior in its organisms consists in the habituation to any stimulus which is repeated at sufficiently close intervals so that the organism no longer responds to it. This is shown even among the protozoa. A *Stentor* or a *Loxophyllum* subjected to a light mechanical stimulus at short intervals soon fails to respond as at first, but the duration of the modification so produced is very short; in *Loxophyllum* it probably does not extend over two or three seconds. Similar effects of repeated stimulation but of longer duration have been observed in *Hydra,* several species of sea-anemones, planarians, annelids and various other lower invertebrates. As a rule failure to respond may occur more quickly and the effects of the stimulus remain longer as we pass up the scale of animal life.

Occasionally the reverse phenomenon occurs when the response to a given stimulus is increased instead of diminished with repeated applications—a result which suggests the effect of the summation of stimuli. At times, as Bohn found in *Cerianthus,* there is an initial increase of responsiveness followed by a dulling of sensitivity. Bohn has attempted to subsume the effects of repeated stimulation under a general "law" to the effect that stimulation always produces at first increase of sensitivity to be followed later by a decrease. Sometimes, as Bohn claims, the initial increase is so short as to escape detection; which may be true, but the burden of proof is on M. Bohn.

Repetition of a stimulus may call forth not only quantitative differences of response, but it may evoke responses of very different character. Animals are frequently provided with several modes of reacting to a given stimulus which may be called into play one after the other. Jennings has shown that if a *Stentor* is subjected to a light mechanical stimulus by causing fine particles of India ink to fall upon its disk from a capillary pipette it

usually reacts first by bending a little to one side. If the particles continue to fall on the disk the beat of the cilia covering the body may suddenly be reversed, thus creating a current tending to carry the offending particles away. If in spite of this the particles still impinge upon the disk the *Stentor* may contract one or more times. Finally, if all these reactions are tried in vain the infusorian may give a number of violent contractions, break loose from its place of attachment, and swim away.

It would be an error to interpret the varied behavior of this unicellular organism as a manifestation of intelligence, although it is not unlike what the behavior of an intelligent creature might be under the circumstances. No power of learning by experience has ever been discovered in *Stentor,* or indeed in any other protozoan. The organism is provided with a number of different modes of response, and which one is set in action depends upon internal factors which are influenced by the creature's previous activity. The organism which has responded to a stimulus has become transformed into a different mechanism which may respond more or less readily than before or radically change its method of behavior.

A striking illustration of varied responses to a given stimulus has been described by Jennings in the sea anemone *Stoichactis.* If a foreign body is placed upon its disk the anemone tries to rid itself of the object in various ways. The tentacles near the object collapse and the area between them extends, thus producing a relatively smooth surface so that the waves can readily wash the object away. If this does not occur the region under the object begins to swell, thus rendering the removal of the object still easier. If this reaction is unsuccessful the edge of the disk begins to sink so that a smooth sloping surface is formed from which the object can readily slide. Here, as in the case of *Stentor,* we have an organism capable of reacting in several ways to a given stimulus. What particular reaction is evoked depends upon previous stimulations.

Modification of behavior caused by different conditions of nutrition are found in the lowest members of the animal kingdom. Even the white blood cells after they have ingested a number of bacteria refuse to take in more. Whether there is a limit to the appetite of *Amœba* has not been determined, but many infusorians such as *Stentor,* after having swept in a certain amount of food, react to food particles in a quite different way than when in a hungry condition. *Hydra* when not fed for some time extends the body, sways about in various directions and keeps up a restless movement of its tentacles, thereby increasing its chances of contact with the small creatures which serve as its prey.

Instances of the non-intelligent modifications of behavior might be multiplied indefinitely. As we pass to higher forms the capacity for responding in different ways to a given situation becomes greatly increased. "Nature," says James in his admirable chapter on instinct, "implants contrary impulses to act in many classes of things, and leaves it to slight alterations of the conditions of the individual case to decide which impulse shall carry the day," and he points out that many animals lose the instinctive demeanor and appear to lead a life of hesitation and choice, not because they have no instincts, but because they have so many of them that they block one another's path. Intelligence in the acceptation of the term which we have accepted begins with the formation of associations. It does not make its appearance, so far as is known, until a comparatively

high stage of organization has been attained. The evolution along the lines of complexity of instinct and ready modifiability of reactions to suit new conditions, affords a substantial basis for intelligent behavior. Without such evolution the power of associative memory would avail little. But with a large number of readily modifiable instincts, associative memory becomes the means of affording a much wider and closer adjustment to the environment.

The studies which have been made of primitive types of intelligence such as found in crustaceans, fishes and amphibians have shown that associations are formed by a gradual process of reinforcement or inhibition of a particular reaction to a given stimulus. The method followed is one which Lloyd Morgan has designated as "trial and error." It may be illustrated by the experiment of Yerkes on the formation of associations in the crayfish. In these experiments a box was employed into one end of which the crayfish was admitted through a narrow aperture. The other end of the box was divided by a median partition which gave the crayfish a choice of two routes to a tank of water at the other end into which the creature was naturally desirous of getting. One of the two ways to the water was closed by a glass plate at its farther end so that the crayfish was afforded a choice of a right and a wrong path to the water. Would the crayfish after a number of trials learn to choose the right path and avoid the closed passage? In the first ten experiments the crayfish went as often to the right as it did to the left, but in the next ten trials the percentage of correct choices was somewhat greater. Finally after a large number of trials the animal came to choose the right path to the water, making but rarely any mistakes.

Similar experiments with crabs, fishes and the frog have yielded similar indications of slow learning. In some respects such learning resembles the slow formation of a habit rather than the judgment of a consciousness which "sizes up" the situation and determines upon a certain course of action. It is quite probable that such a primitive form of learning does not include any association of ideas. It can be satisfactorily accounted for by assuming nothing more than an association of certain sense perceptions with particular movements. The animal may have no ideas to associate—nothing but sense impressions and motor impulses. Of course its mental content *may* include much more than this, but in interpreting the behavior of animals it is generally advantageous to follow the principle laid down by Lloyd Morgan—which is a sort of special application of the law of parsimony—that we should not assume the existence of a higher psychic function if the phenomena can be explained as well in terms of a lower one.

The step from sensori-motor association to the association of ideas is not, I believe, a wide one, and comes about as a natural consequence of the elaborateness and what Hobhouse has designated as the "articulateness" of the mental process of adjustment. It is foreign to our purpose, however, to trace the increase in the number, delicacy, quickness and complexity of the processes of association which we meet in the various stages of mental evolution. One problem at present lies in the initial step involved in the formation of a simple association. And it is a problem which, despite its apparent simplicity, involves the consideration of some vexed and subtle questions.

In learning we have to do with two opposite processes of reinforcement and inhibition. A chick after it pecks at a caterpillar which is wholesome and savory pecks

at a similar caterpillar more readily on a second occasion. Something has apparently reinforced the connection between the visual impression produced by the caterpillar and the pecking impulse. If, on the other hand, the chick pecks at a caterpillar having a nasty taste it is apt to avoid pecking at it a second time. Something has happened to inhibit the response that would otherwise occur. We commonly explain such behavior by ascribing to the creature feelings of pleasure and pain. We say that the chick pecks at one kind of a caterpillar because of the pleasant taste it derives, and avoids another variety because its taste is bad. Pleasure and pain apparently function as agents for the reinforcement of certain reactions and the stamping out of others. It is a general rule, though not without certain exceptions, that what affords pleasure is conducive to organic welfare, while that which is productive of pain is injurious. The upshot is that the associations that are the outcome of the pleasure-pain response are of just the kind that minister to the animals' needs. Beneficent arrangement! Apparently we have to do with a selective agency which preserves and intensifies certain kinds of behavior and rejects others on the basis of their results—a kind of "sorting demon" in the realm of behavior. What could be more teleological!

The fact that what is pleasant is usually beneficial and what is painful is usually injurious may be explained with some plausibility as the result of natural selection, as was first contended by Herbert Spencer. Animals which took pleasure in doing things which were bad for them and which experienced pain in doing things which were good for them would be very apt to fare ill in the struggle for existence. Natural selection would ever tend to bring about a condition in which the pleasant means the organically good and the painful means the reverse. We should not expect the correspondence, if brought about in this way, to be complete, and it is rather in favor of the theory that we do not find it so.

But granting this contention of Spencer, there is the important question still left unanswered, namely, Why do animals follow what is pleasant and avoid what is painful? In other words, why does pleasure reinforce and why does pain inhibit? Here is another fundamental problem and we find that Spencer with his usual appreciation of fundamental problems was on the ground early with a theory. Pleasure, according to Spencer, is the concomitant of a heightened nervous discharge; pain the concomitant of a lessened nervous discharge. An act which brings pleasure causes an influx of nervous energy to the centers concerned in the movement; the lines of discharge become "more permeable," and upon a repetition of the conditions the same act follows with greater readiness than before. If the act is followed by pain with its concomitant of lessened nervous discharge, the diminution of nervous energy serves to prevent the performance of the act in response to the same conditions. Closely similar explanations of the physiology of the pleasure-pain response have been given by Bain and by Baldwin, the latter declaring that "pleasure and pain can be agents of accommodation and development only if the one, pleasure, carry with it the phenomenon of motor excess—and the other, pain, the reverse—probably some form of inhibition or of antagonistic contraction."

The physiological concomitants of pleasure and pain have afforded a subject for numerous laboratory studies and almost no end of theories. It has been impossible thus far to discover that either of these states is invariably accompanied by any

definite physiological condition. The theory of Spencer and Bain is open to obvious criticism, for the man who steps on a tack undoubtedly has a "heightened nervous discharge," as much as a man who shouts for joy. And I believe I am safe in saying that no theory of the physiology of pleasure and pain is on a sufficiently firm basis to warrant its being regarded as anything more than a very tentative working hypothesis.

With our present knowledge of the psycho-physiology of pleasure and pain, the attempt to explain how these states or their physiological concomitants, whatever they may be, can act as agents of reinforcement and inhibition seems rather a fruitless one. The process which we meet at the beginning of intelligence in simple associative memory may be formulated as follows:

stimulus — reaction — pleasure — reinforcement
physiological state x
stimulus — reaction — pain — inhibition
physiological state y

Spencer, Bain and others have endeavored to show how the organic accompaniments of pleasure and pains modify the creatures' subsequent responses. But as the problem was interrupted by these writers our ignorance concerning the physiological states x and y brings us to a standstill.

In his valuable work on "Mind in Evolution" Hobhouse has presented a new point of view in considering this problem, which has the advantage of not involving any general theory of the physiology of pleasure and pain. It is essentially a theory of how behavior comes to be adaptively modified through the formation of associations. It makes no attempt to explain why pleasure is associated with certain experiences and pain with others. Such association may turn out to be as inexplicable as the problem why stimulation of the optic nerve gives rise to a sensation of light instead of some other kind of feeling. What it is feasible to attempt to explain is why certain responses tend to be repeated and others tend to be inhibited. And this can be explained with some plausibility as due to the congruity or incongruity of the reactions which come to be associated. For the sake of illustration let us consider again the chick which pecks at a nasty caterpillar. The irritation set up by the caterpillar in the chick's mouth evokes movements of withdrawal and ejection. The two responses of pecking and ejection become associated, but as the two movements are contradictory the result is inhibition. The pecking reaction no longer occurs in the presence of a second nasty caterpillar, not because of any stamping-out influence of the physiological concomitant of pain, but because it becomes joined with an antagonistic reaction.

In a previous paper by the writer the attempt was made to extend the theory of Hobhouse to account for the reinforcement commonly held to be caused by pleasure. The assumption was made that this process is due to an organic congruity of the reactions. If the caterpillar pecked at is a savory one there is set up the reflex of swallowing. Pecking and swallowing form the normal elements of a chain reflex; when one part of the structure concerned is excited it tends to increase the tonus of the associated parts, and thus reinforce the original response. I have found that in the crayfish stimulation of the antennules, which are important organs of smell, sets up chewing movements of the mouth parts and grasping movements of the small chelæ. Similarly stimulating the small chelæ evokes chewing movements of the mouth parts and twitching of the antennules, while stimulating the mouth parts

directly may cause movements in both the other sets of organs. We have here as a matter of fact a number of reflexes which mutually reinforce one another. Suppose that in the chick the sight-pecking response and the taste-swallowing response are related as the feeding reflexes demonstrably are in the crayfish; the second response would thus tend to reinforce the first, and if this tendency persisted we would have a case of learning by experience.

Animals in the course of their instinctive responses encounter stimuli which bring about other responses. These become associated. According to the nature of the nervous pathways involved, there may be reinforcement of or interference with the original reaction. Experience brings about an extension of the range of adaptations by the assimilation of congruent reactions and the elimination of acts whose secondary consequences are in the nature of antagonistic and thereby inhibitory responses. Such we may say, by way of expressing a tentative view-point, is the nature of primitive intelligence.

But it will be seen that the capacity to form new adaptations rests upon the primary adaptiveness of the instinctive reactions. The power of formation of associations alone would never lead to improvement. The adaptiveness of intelligence is based upon the adaptiveness of instinct; it may be said that intelligence is a means of enabling an animal to live its life more completely and successfully, but instinct furnishes the fundamental springs of action. Even complex creatures like ourselves form no exception to this rule.

S. J. Holmes
University of Wisconsin

THE GALTON CHAIR OF EUGENICS

We have noted that Sir Francis Galton, F.R.S., who died on January 17, aged 88, had left his residuary estate to the University of London for work in eugenics. This residuary estate will amount to about £45,000. In his will Sir Francis Galton describes the scope of his new foundation as follows:

I devise and bequeath all the residue of my estate and effects, both real and personal, unto the University of London for the establishment and endowment of a professorship at the said university to be known as "The Galton Professorship of Eugenics," with a laboratory or office and library attached thereto. And I declare that the duty of the professor shall be to pursue the study and further the knowledge of national eugenics—that is, of the agencies under social control that may improve or impair the racial faculties of future generations physically and mentally. And for this purpose I desire that the university shall, out of the income of the above endowment, provide the salaries of the professor and of such assistants as the senate may think necessary, and that the professor shall do the following acts and things, namely:

1. Collect materials bearing on eugenics.
2. Discuss such materials and draw conclusions.
3. Form a central office to provide information, under appropriate restrictions, to private individuals and to public authorities concerning the laws of inheritance in man, and to urge the conclusions as to social conduct which follow from such laws.
4. Extend the knowledge of eugenics by all or any of the following means, namely: (*a*) professorial instruction; (*b*) occasional publications; (*c*) occasional public lectures; (*d*) experimental or observational work which may throw light on eugenic problems.

He shall also submit from time to time reports of the work done to the authorities of the said university.

I also declare that the said university shall be at liberty to apply either the capital or income of the said moneys for any of the purposes aforesaid, but it is my hope that the university will see fit to preserve the capital thereof wholly or almost wholly intact, not encroaching materially upon it for cost of building, fittings or library. Also that the university will supply the laboratory or office at such place as its senate shall from time to time determine, but preferably in the first instance in proximity to the Biometric Laboratory. I state these hopes on the chance of their having a moral effect upon the future decisions of the senate of the university, but they are not intended to have

any legally binding effect whatever upon the freedom of their action. And I declare that it shall be lawful for the senate of the said university, if they shall think fit so to do, to postpone the election of the first or any subsequent professor of eugenics for a period of not exceeding four years from the date of my death, or from the date of the occurrence of any vacancy in the office as the case may be. . . .

I declare it to be my wish, but I do not impose it as an obligation, that on the appointment of the first professor the post shall be offered to Professor Karl Pearson, and on such conditions as will give him liberty to continue his Biometric Laboratory now established at University College.

SCIENTIFIC NOTES AND NEWS

THE National Academy of Sciences will hold its annual meeting in Washington, beginning on Tuesday, April 18.

THE American Philosophical Society will hold its general meeting in the hall of the society at Philadelphia on April 20, 21 and 22. On the evening of April 21, Professor Svante Arrhenius, of Stockholm, will give an illustrated lecture on the physical condition of the planet Mars, which will be followed by a reception in the hall of the College of Physicians. On the evening of the twenty-third the annual dinner of the society will take place at the Bellevue-Stratford.

DR. WILLIAM H. WELCH, professor of pathology in the Johns Hopkins University, has received from the emperor of Germany the decoration of the royal crown of Prussia, second class.

DR. LEWIS BOSS, director of the Dudley Observatory and of the department of meridian astronomy of the Carnegie Institution, has been elected a corresponding member of the Academy of Sciences of St. Petersburg.

DR. S. W. STRATTON, director of the Bureau of Standards, represents the United States government at the International Convention on Weights and Measures at Paris, beginning on March 29.

PROFESSOR ALBERT A. MICHELSON, head of the department of physics in the University of Chicago, will give a course at the University of Göttingen in the coming summer semester.

AT the invitation of the Department of Education of the Philippine government, Professor J. Paul Goode, representing the University of Chicago, will deliver a series of lectures to the Teachers' Assembly at Bagino in May.

THE expedition that was sent to Argentina two years and a half ago under the auspices of the Carnegie Institution for the purpose of making meridian measurements of position of stars down to the seventh magnitude that are south of $-20°$ of declination, and generally inaccessible for exact measurement at observatories of the northern hemisphere, has completed its meridian-work. In 1909 and 1910 about 87,000 meridian-determinations of positions were made with precision. The observations were conducted on a fundamental basis, and correspond to others to be secured at the Dudley Observatory at Albany as an integral part of the entire enterprise. The instruments were shipped to Albany from Buenos Aires early in March and the members of the staff, of which Professor Richard H. Tucker is director, are arriving at various times.

THE magnetic survey yacht the *Carnegie* arrived at Capetown on March 20, having completed successfully a voyage of about 14,000 miles in the Atlantic Ocean since last June. Dr. Bauer left Vancouver on March 24 to make magnetic observations in the Samoan Islands during the total solar eclipse of April 28 and to meet the *Carnegie* at Colombo, Ceylon.

IT is stated in *Nature* that at the recent meeting of the Australasian Association for the Advancement of Science in Sydney, the Mueller memorial medal was awarded to Mr. Robert Etheridge, curator of the Australian Museum, in recognition of the value of his numerous contributions to the paleontology and ethnology of Australasia.

THE trustees of Dartmouth College have voted that Charles Henry Hitchcock be made Hall professor of geology, emeritus, and that Gabriel Campbell be made Stone professor of

intellectual and moral philosophy, emeritus. They retired last year on the Carnegie Foundation fund. Frank Asbury Sherman, professor of mathematics, will retire at the end of the academic year, when he will have reached the age of sixty-nine years.

Professor Bernstein, director of the physiological laboratory at Halle, will retire from active service at the beginning of the winter semester on account of advanced age.

Dr. Rollin T. Chamberlin, of the University of Chicago, sailed for South America on March 20, to engage in geological work on some of the metamorphic rocks of southeastern Brazil and on certain portions of the Andes.

Dr. Simon B. Wolbach, assistant professor of bacteriology at the Harvard Medical School, and Dr. J. L. Todd, of McGill University, have departed for West Africa to study the sleeping sickness and allied diseases.

Dr. A. S. Pearse will leave the University of Michigan on April 1, to fill a position as assistant professor in the department of zoology of the University of the Philippines in Manila.

Dr. Osten Bergstrand, for some time observer at the Upsala Observatory, Sweden, has been appointed professor of astronomy in the Upsala University and director of the observatory.

Dr. Svante Arrhenius, director of the Nobel Institute, Stockholm, lectured before the Washington Academy of Sciences and the Philosophical Society of Washington on March 25 on "The Siderial Cultus."

Dean Allen J. Smith, of the University of Pennsylvania, closed a series of popular health lectures, given under the auspices of the medical faculty, March 17. His talk was on "Flies and Other Insects as Carriers of Disease."

On the evening of March 17, Professor C. J. Keyser, of Columbia University, delivered a lecture before the Philosophical Club of Princeton University on "The Nature and Philosophic Significance of the Mathematical Doctrine of Invariance."

Mr. James R. Steers, of the class of 1853, has made a further gift of $2,200 to the Wolcott Gibbs Library of Chemistry of the College of the City of New York. The money is to be expended in the completion of certain journals and for cataloguing.

A portrait of David Rittenhouse, painted in 1772 by Rembrandt Peale, has been given to the University of Pennsylvania by Mrs. William Lawber. David Rittenhouse was professor of astronomy and vice-provost of the university from 1779 to 1782, and from 1782 until the time of his death in 1796, a trustee.

Félix Plateau, who recently retired from active duties of the chair of zoology in the University of Genth, known especially for his studies on the behavior of insects, died on March 4.

In the New York senate on March 21 a bill was introduced to incorporate "The Carnegie Corporation of New York." The incorporators named in the bill are Andrew Carnegie, Senator Elihu Root, president of the Carnegie Endowment for International Peace; Dr. Henry S. Pritchett, president of the Carnegie Foundation for the Advancement of Teaching; William H. Frew, president of the board of trustees of the Carnegie Institute of Pittsburgh; Robert S. Woodward, president of the Carnegie Institution of Washington; Charles L. Taylor, president of the Carnegie Hero Fund Commission; Robert A. Franks, president of the Home Trust Company, and James Bertram, Mr. Carnegie's secretary. Under the language of the bill the incorporators are authorized "to receive and maintain a fund and apply the income to promote the advancement and diffusion of knowledge among the people of the United States, by aiding technical schools, institutions of higher learning, libraries, scientific research, hero funds, useful publications, and by such other agencies and means as shall from time to time be found appropriate."

The Austrian Academy of Sciences held on March 9 a sitting to celebrate the fiftieth anniversary of the appointment of the Archduke Rainer to be its curator. The archduke gave 100,000 kronen to the academy as an en-

dowment to keep the academy in touch with the progress of science abroad.

M. FAUVEL has offered 30,000 francs for the construction of an addition to the laboratory of the National Museum of Natural History in Paris.

THE president of the Royal Society and the members of the General Board of the National Physical Laboratory visited the laboratory on March 17.

AT midnight on March 10 the clocks at the railway stations and all government offices and municipal buildings of France and Algeria were set back nine minutes twenty-one seconds, to bring them in accord with Greenwich time, which is now used in all countries of western Europe except Russia, Portugal and Ireland.

THE ninth International Congress of Agriculture will be held at Madrid, beginning on May 8, under the patronage of his Majesty Alfonzo XIII. Americans wishing to attend the congress may secure the printed account of the arrangements from the American member of the *Commission Internationale d'Agriculture*, Dr. L. O. Howard, U. S. Department of Agriculture, Washington, D. C.

THE next triennial prize of £300, under the will of the late Sir Astley P. Cooper, will be awarded to the author of the best essay or treatise on "The Means by which the Coagulability of the Blood may be Altered." Essays, written in English, must be sent to Guy's Hospital, before January 1, 1913.

THE U. S. Civil Service Commission announces an examination on April 15, to fill at least three vacancies in the position of magnetic observer in the Coast and Geodetic Survey. The initial salaries will range from $75 to $90 a month, according to the character of the work and the qualifications of the applicant; and in exceptional cases where the person employed has had repeated experience in magnetic work, the initial salary may reach $125 a month. Appointments to permanent positions are also made from the examination for laboratory assistant in the Bureau of Standards.

THE Harpswell Laboratory will be open this summer for the fourteenth season, from June 19 to September 15. South Harpswell is 16 miles from Portland, Maine, with which it is connected by several boats daily. The location is admirably adapted for a biological station. It has a very rich marine flora and fauna. Especially noteworthy are the abundance of Elasmobranch material, and the eggs of *Cerebratulus* and *Echinarachnius*, so valuable for experimental studies. For several years the laboratory has been open solely for research, no instruction being given. It has nine private rooms, several of which are already engaged for the coming summer, and can accommodate a few more in the large laboratory room. Thanks to the cooperation of several colleges and universities, the laboratory is able to offer its facilities free to those competent to carry on independent investigation. There will be a collector employed and the laboratory has several boats, including a motor boat, dredges, the usual instruments and glass ware and a little physiological apparatus. Alcohol for collections and expensive chemicals can not be furnished. There is also a small library. The laboratory will not issue a circular this year. Those wishing to avail themselves of its facilities should apply to Professor J. S. Kingsley, Tufts College, Mass. It would be well to give some idea of the line of work to be followed so that advice may be given as to the prospects of material.

BY cooperation between the American Museum of Natural History and the United States Bureau of Fisheries, the government steamer *Albatross* sailed from San Diego, on February 25, on a two months' collecting expedition to Lower California. Dr. Charles H. Townsend, acting director of the museum, is in command of the expedition. He is accompanied by seven investigators and collectors, representing the U. S. National Museum, the New York Zoological Society and the New York Botanical Gardens, all of which bear a share of the expense of the trip. Dr. Townsend began the work with a line of deep-sea dredgings to Guadalupe Island, some two

hundred and fifty miles from San Diego. From Guadalupe Island the *Albatross* planned to work eastward to begin a fish survey of the peninsula of Lower California. The fishery resources of the region will be studied with a view to the establishment of closer fishery relations with Mexico. There will be work on shore also. The peninsula is seven hundred and fifty miles long and will be studied along both coasts. During the progress of the vessel along these coasts collecting parties will be landed each day to procure the mammals, birds, reptiles and fishes of the region, which are of especial interest to naturalists because so large a number of them are peculiar to the locality.

The Charleston (S. C.) Museum has been developing a large amount of public interest recently in the scientific studies that were so much cultivated in that city in the years before the war. In the middle decades of the last century, Charleston was a center of scientific work and interest. The visit there of Agassiz in 1850, and the notable work of Audubon and Bachman, and of others beside, gave great stimulus to scientific pursuits. There has been organized in recent years a Natural History Society, as an educational department of the museum, composed largely of young people, but by no means confined to them, which has already done much excellent local work, and is raising up and training a body of enthusiastic observers, especially in botany and ornithology. The museum has also within the past year issued the first volume of its "Contributions,"—"The Birds of South Carolina," by Arthur Trezevant Wayne, honorary curator of ornithology in the museum, and a recognized authority on the birds of the region. This is an octavo volume of 254 pages, and is the most complete and accurate work on the subject that has been published. With a view to stimulating public interest, and also in the desire to link the present and future work of the museum with that which gave Charleston its reputation in the days of Audubon and Bachman, the Natural History Society proposed to devote its March meeting to the work of those eminent men, and to make it a memorial occasion. To this end, request was made for copies in private hands, of the plates and various editions of their works, and also for other objects connected with them. The main hall of the museum was assigned to this Audubon-Bachman exhibit. The result exceeded all anticipation, and revealed a wealth of choice material of this kind, belonging to private owners that had not been suspected. A noble collection of Audubon's bird-plates was thus secured, together with copies of the several editions of his work on birds, and of the joint work of Dr. Bachman on the mammals, with busts, portraits, original drawings, etc., of great interest. This loan collection attracted much public attention, and has given a distinct impetus to scientific appreciation in the city. At the memorial meeting, held on March 2, a large audience was present, and very interesting addresses were given on the life and work of both Audubon and Bachman, by several speakers, including Director Rea, of the Museum, and the venerable Dr. C. S. Vedder, of Charleston, an intimate friend of Dr. Bachman for many years.

A new expedition is being arranged, Reuter's Agency learns, to explore the Snow Mountains of New Guinea, a region in which the British expedition under Captain Rawling is at present working. The Dutch government is interested in the undertaking, and Queen Wilhelmina and Prince Henry have contributed towards the cost. The details have not yet been finally arranged, but it is probable that Captain von Nauhuys, who has already been on three expeditions in New Guinea under Dr. Lorentz, will be in command and will be accompanied by a number of European scientific men.

The production of tungsten in the United States increased considerably during the year 1909, but practically all the nickel, cobalt and tin consumed was imported. In an advance chapter from "Mineral Resources of the United States Calendar Year 1909," prepared by Frank L. Hess for the United States Geological Survey, the production of these metals and of the rarer metals vanadium, titanium,

molybdenum, uranium and tantalum is reported. Colorado led in the production of tungsten in 1909. The prices quoted for the metal were considerably higher than in 1908, and the production of ore increased from 671 short tons, valued at $229,955, to 1,619 tons, valued at $614,370. This is the largest quantity produced by any country in one year. The United States last year produced nearly one third of the world's total production of tungsten ore, which was about 5,300 tons. Very little nickel was produced in the United States, but a great deal was imported, and, as more nickel is refined in this country than can be used here, large quantities were exported. In all, 22,194,102 pounds of nickel as metal and as metallic content of ores were brought into the United States in 1909. The total value was $3,036,273. The exports of nickel amounted to 12,048,737 pounds, valued at $4,101,976. The importations of cobalt amounted to 12,132 pounds, valued at $11,696. Titanium is being used with great success in making steel rails. The New York Central Line has given rails treated with ferrotitanium a thorough test in the Grand Central terminal yards and now requires that all rails made for its lines shall be treated with titanium alloy. Rutile, or titanium oxide, was produced in the United States last year only at and near Roseland, Va. No rutile was imported, but one American company exported it in considerable quantities. Molybdenum, uranium and tantalum were little used in the United States in 1909, and only a small quantity of tantalum was produced. A little was imported from Germany, but the use of tungsten in incandescent lamps has proved so satisfactory that the demand for tantalum has probably not greatly increased. Although the United States uses 43 per cent. of the world's output of tin, it is not an important producer of that metal. Tin was produced in Alaska and South Dakota in 1909, but not in large quantities. In 1909 there were imported into the United States 47,662 tons of tin, valued at $27,558,546. The average price was 28.91 cents a pound. Several thousand tons of tin are recovered from used tin cans and other wastes.

UNIVERSITY AND EDUCATIONAL NEWS

THE subscription to the memorial to President Grover Cleveland exceeded $100,000 on the seventy-fourth anniversary of his birth. It will be remembered that the memorial is to be a tower forming part of the graduate college of Princeton University.

THE University of Washington at Seattle has received from Messrs. Sigmund and Abraham Schwabacher $30,000 to maintain a bureau of child welfare.

GOVERNOR STUBBS, of Kansas, has vetoed the bill placing the University of Kansas and other state institutions under the control of a commission consisting of three paid members. It is stated that the recent legislature seriously reduced the appropriations for the university.

THE University of the Pacific, College Park, California, by the action of its trustees, will henceforth be known as the College of the Pacific. This action separates the college from a conservatory of music and a preparatory department.

AT the meeting of the board of regents of the University of Minnesota on March 3, the salary of one of the professors, who is dean of one of the schools, was increased from $4,000 to $6,000.

THE cornerstone of the administration building of the William M. Rice Institute, was laid by the trustees on March 2, the seventy-fifth anniversary of Texan independence. The seven members of the board were present. They are: J. A. Baker, W. M. Rice, Jr., J. E. McAshan, B. B. Rice, C. Lombardi, E. Raphael, E. O. Lovett.

HEREAFTER the degree of bachelor of chemistry (B.Chem.) will be conferred by Cornell University on students who have completed the special course in chemistry. The department of chemistry remains a department of the College of Arts and Sciences. A four-year course in chemistry and allied subjects has been offered by the department since about 1903 for students planning to follow chemistry as a profession. Although the course is essentially technical in character and professional in purpose, the degree of bachelor

of arts, has heretofore been conferred upon its graduates.

At Smith College Harriet W. Bigelow has been promoted to be professor of astronomy, and Frances Grace Smith to be associate professor of botany.

At Yale University Dr. Alexander Petrunkevitch has been promoted to be assistant professor of zoology, and Dr. Carl Johns, to be assistant professor of chemistry.

DISCUSSION AND CORRESPONDENCE

THE AIR WE BREATHE

To the Editor of Science: As a member of the American Society of Heating and Ventilating Engineers, who had the pleasure of hearing the recent address of Dr. Gulick before that society, I desire to reply to his letter in Science of March 3. I believe that Dr. Gulick is engaged in a research whose results may be of the utmost importance to the health of a large fraction of the human race—namely, the children in the schools—and it is to be desired that he be given every encouragement to continue in it.

First, to answer some of his questions in regard to the physics of atmospheric air. No one knows "the reason why" raising the temperature of air increases its capacity for holding moisture. It is merely one of the great facts of nature, like gravitation, and like the fact that water freezes at 32° F. At 32° F. a cubic foot of air has the capacity of holding in a gaseous condition 0.0003 pound of water; at 62°, 0.00087 pound, at 72°, 0.00121 pound, at 102°, 0.003 pound, and so on.

"Is there any difference between steam and humidity?" Steam is water in a gaseous state. There is no difference between steam and the gaseous or uncondensed vapor of water in the atmosphere. When steam escapes from a pipe into an atmosphere colder than itself it condenses into fog, which is visible, but if the atmosphere is not saturated with moisture it will rapidly dry the fog, turning it again into invisible vapor. Humidity is the condition of the atmosphere as regards moisture. Relative humidity is the ratio or percentage that the moisture contained in the atmosphere bears to the maximum quantity it can contain at the same temperature. Thus if a cubic foot of air at 62° contains 0.00087 pound of water vapor, the air is "saturated" and the relative humidity is 100 per cent., but if the same quantity of moisture is contained in a cubic foot of air at 72° the relative humidity is only $0.00087 \div 0.00121$ or 72 per cent.

"The manuals of the heating and ventilating engineers tell us that with a good system of ventilation the opening of windows causes only danger; yet as a matter of fact, children in rooms so treated do not exhibit the distressing conditions," etc.

The "danger" from opening windows is not to the children in the room in which the windows are opened, but to the children in the adjoining rooms in which the windows are not opened. In the fan-blower or "plenum" system of ventilation the entrance and exit flues and dampers are so designed and adjusted as to cause each room to receive its due proportion of the total air supply. If a window is opened in one room (unless the wind is blowing towards the window) the resistance to the passage of air from the room will be less than if only the exit flue were open; consequently there will be a lower static pressure in this room than in the other rooms, and it will receive from the entrance flue more than its proper share, thus robbing one or more of the other rooms of their share. The flow of air in a complex system of piping is like the flow of water. If in an apartment house with ordinary plumbing a tenant on a lower floor draws hot water into a bath tub, he will rob the bath room on the floor above of its hot water supply for the time being. So in a school-house fitted with air pipes, if the flow of air from the fan into one room is increased by opening a window, there will be a smaller supply for the other rooms. The "danger" therefore is that of unbalancing the ventilating system. This danger would be avoided if there were in use an automatic ar-

rangement for closing the damper of the admission flue every time a window was opened.

The science of heating and ventilation may be divided into two branches: (1) hygiene, (2) engineering. The first includes the knowledge of the effect upon health and physical and mental vigor of the condition of the air in buildings as regards temperature, relative humidity, content of CO_2 and other noxious gases, etc. The second includes the knowledge of how to design, install and operate apparatus which will maintain the conditions that are desired. The engineering branch of the science is in fairly good shape. If the temperature, humidity and CO_2 desired in a given school room are specified, the engineers can furnish apparatus which will meet the specifications, and whether or not the specifications are fulfilled can be tested by means of thermometers, hygrometers and analyses of the air in the rooms. The hygienic branch, however, is in a very poor shape. It is for the doctors to settle, and not for the engineers.

In Dr. Gulick's address he threw doubts "upon the very foundations upon which the science of heating and ventilation is built," that is, the hygienic foundations. He doubts, I understand, if it is necessary to supply sufficient air to keep the CO_2 down to 8 parts in 10,000, the recognized standard. All that the engineers know is that there is a tradition, handed down in the text-books from time immemorial, that the CO_2 should not exceed this limit, and on the generally accepted statements that a man will exhale on the average 0.6 cubic foot of CO_2 per hour, and that the outside air contains about 4 parts in 10,000, the requirement will be met by furnishing 1,500 cubic feet of air per hour to each inmate. To be on the safe side, the school authorities in Massachusetts many years ago, placed in their code the specification that 1,800 cubic feet per hour should be supplied for each pupil, and this requirement has been placed in the statute laws of several states.

As to the desired temperature and humidity, and as to the desirability of having artificial ventilation at all, the doctors disagree. Dr. Gulick's letter states that Doctors Thompson and Brennan "think we ought to do away all systems of ventilation, and use simply natural ventilation—open windows," while Dr. Leonard Hill has found an "admirable result" from a plenum system, giving a moving air at 57–60° F. and about 70 per cent. relative humidity with all windows and doors kept closed. Both of these ideas are opposed to the modern American system, which is a plenum system maintaining a temperature of 70° F., letting the humidity be what it may, with no attempt to control it. A relative humidity of 70 per cent. at 60° F. is about equivalent in actual quantity of moisture to 50 per cent. at 70° F., and this is probably much greater than the humidity in New York schools in clear cold weather.

The temperature that is desirable in school rooms is probably largely a matter of habit and local custom. Our people are accustomed to a room temperature of 68–72°, and think they like it. Englishmen in their own country profess to like 58–62°. Which is actually the best temperature, or whether the higher temperature is better here and the lower in England probably no one knows. As to the effect of humidity at temperatures between 58° and 72° F. does any one really know? We do know that high humidity at 80° is much more uncomfortable than low humidity at 90°, and we know also that regions of low humidity are famous as health resorts.

There is a vast amount of ignorance as to the hygiene of ventilation. Dr. Gulick seems to have an arsenal of facts (?) and a body of "as yet undigested information" on the subject, and it is to be hoped that he will correlate and digest them, and present them in digested form before some learned society and have them discussed and printed for the public benefit. I venture the opinion, however, that all the investigations by the authorities he names, after being correlated and digested, are insufficient in extent and not sufficiently scientific in quality to form the basis of a final judgment on the disputed questions in the hygiene of ventilation.

What is needed is a new research, aided by all the facilities of modern science and instru-

ments, under the auspices of the Carnegie Institution or the Russell Sage Foundation, to discover by direct experiment on a large scale, the effect upon the health and comfort of school children, of the three different systems of ventilation: (A) Open windows, as recommended by Doctors Thompson and Brennan, (B) the plenum system recommended by Dr. Hill, with low temperature and high humidity, (C) the American plenum system with 70° temperature and low humidity.

For the carrying out of a portion of the research, I suggest that experiments be made in several rooms of a large grammar school, all with the same window exposure and light, one room being treated by direct radiation and all ventilation obtained by open windows, and others by the plenum system, with the volume, temperature and humidity regulated at will. Let the rooms be thoroughly aired, say from 8 to 8.30 A.M., then when the children enter at 9 A.M. have their condition noted by trained specialists, with all the known pathologic and psychopathic tests, including the use of instruments for recording the pulse and the respiration, "reaction time," etc., and let these tests be repeated at noon. Tests should also be made to determine relative mental fatigue, lack of attention, etc. Statistics should be collected to determine what relation, if any, exists between the ventilating system or quality and quantity of ventilation in the schools and the prevalence of adenoids, sore-throat, headache, colds, etc. It may be found that these troubles have relation to the environment of the child during the nineteen hours that he is out of school rather than the five hours that he is in school.

Experiments should be made to find whether the percentage of CO_2 in the school room has the relation to the child's health or mental vigor that it has been commonly supposed to have, and whether an air supply of 600 or 1,200 cubic feet per hour per pupil, instead of 1,800, produces any bad effects. Bacterial examination of the air should be made at the same time.

Such a research as is suggested will take a long time and will cost much money, but is there any investigation now under way that is more worth the money, or that promises more for the welfare of the race?

William Kent

To the Editor of Science: I am writing in the endeavor to clear up some of the elementary questions asked by Dr. Luther H. Gulick in his letter "The Air We Breathe," published in Science of March 3.

When air is completely saturated with water vapor, the humidity in it is vapor, that is, a gas at point of condensation. At other times humidity is a gas, just as much so as CO_2, it is in fact superheated steam in the air. A gas just ready to condense to liquid is called vapor; thus, ordinary steam is a vapor, while superheated steam is a gas, obeying all the well-known gas laws. Humidity is a vapor whenever the air is completely saturated, being then the same as ordinary steam in the air; when, as is usually the case, air is unsaturated, then humidity is superheated steam in the air—a gas. Heat added to vapor superheats it, that is, raises its temperature. Heat taken from vapor condenses some of the vapor to liquid without altering the temperature.

Liquids remain in the liquid state only by virtue of external pressure exerted upon them, and any liquid will quickly turn to gas if all external pressure (which is usually that exerted by the atmosphere) is removed; the tendency for a liquid to gasify depends upon the temperature and kind of liquid, and nothing else.

If a closed vessel containing water and air is let stand at any temperature until conditions have become constant the air will gradually become saturated with water vapor, and the amount of water vapor that will be thus absorbed by a given volume of air will depend only on the temperature of the air; it would, moreover, practically be not only independent of the pressure of the air, but would be the same if another gas or even a vacuum were substituted for the air; also the pressure exerted by the water vapor filling the given space at a given temperature would be the same in all these instances.

In a gas mixture the total pressure exerted by the mixture upon the walls of a containing

vessel is the sum of the pressures exerted by the individual gases composing the mixture; thus, air is a mixture of oxygen, nitrogen, CO_2, superheated water vapor and many other so-called rare gases, and the atmospheric pressure is the sum of all the pressures exerted individually by all these gases.

Take again the closed vessel containing water and air; the quantity of water vapor per cubic foot of space above the water, as well as the pressure exerted by this water-vapor, conditions having become constant, will always be the same for the same temperature, but will rapidly increase with rise of temperature. The following tabulation shows the maximum capacity of air (or other gas or better simply of space) for water vapor at different temperatures, and also the pressures exerted by the water vapor at these maximum capacities, the values being taken directly from the steam tables of Marks & Davis, Table I.

Temperature °Fa	Max. Capacity Lbs./Cu. Ft. Space	Vapor Pressure Lbs./Sq. In.
32	.000304	.0886
40	.000410	.1217
50	.000587	.1780
60	.000828	.2562
65	.000977	.3054
70	.001148	.3626
75	.001346	.4288
80	.001570	.505
85	.001832	.594
90	.002131	.696
95	.002469	.813

To obtain grains/cu.ft. multiply lbs./cu.ft. by 7,000.

The extent to which what Dr. Gulick calls the "thirst" of air for more water vapor is unsatisfied is the difference between the maximum capacity and the quantity actually present. The rapidity with which moisture is evaporated from a moist surface, such as the skin, probably depends, first, on the "thirst" of the air; second, upon the velocity with which the air moves across the moist surface; and third, upon the physical condition of the moist surface, particularly with respect to temperature. Relative humidity is ratio of the quantity of water vapor actually present to the maximum quantity that could be present at a given temperature. Relative humidity will not of itself determine air "thirst," as this "thirst" will be different for the same relative humidity at different temperatures.

Dr. Gulick asks, "Does any one know why delicate children and tuberculous persons get well out of doors but fail to do so indoors." I am not a physician but do believe the air "thirst" is certainly, as a rule, far greater indoors than out of doors, and that this "thirst" continually absorbs moisture from the skin, tending to keep it cold and dry and chilling it in local spots.

WILLIAM J. CROWELL, JR., CH.E.

TO THE EDITOR OF SCIENCE: The capacity of gases for carrying vapors of volatile liquids and the capacity of various volatile liquids to take the vapor form is a subject to which the writer recently has had occasion to devote considerable thought in connection with certain technical processes. Possibly some of the confusion which it seems exists in Dr. Gulick's mind may be dispelled by the following statements which I think can be readily verified.

I will attempt to answer the questions propounded by Dr. Gulick on page 327 of the March 3, 1910, number of SCIENCE.

1. Water vapor, I believe, is the correct term to be used in a discussion of the hygienic effect of atmospheric humidity since the word steam sometimes conveys the meaning of the cloud of minute water particles resulting from condensation of water vapor in the presence of another gas (air).

2. Water vapor then (the amount of which, compared with the total capable of being carried, in the air at any temperature represents its humidity) does "act strictly in accordance with the ordinary laws governing the movement of gases."

3. Humidity in the air is a term used to express the amount of water vapor in the air usually expressed in terms of the percentage of the total amount capable of being carried at that temperature.

4. Water vapor (as long as it remains in

the vapor form) is a gas and obeys all the laws of gases.

5. Air has only the same capacity for carrying any vapor (water vapor) that any other gas has.

6. The amount of humidity (*i. e.*, the basis for calculating percentage humidity) that air, or any other gas, can carry is not a function of the nature of the gas at all.

7. The amount of any vapor that any gas can carry depends solely upon the nature of the substance vaporized and the temperature.

8. The actual percentage by volume of vapor which any gas (air) can carry, *i. e.*, the saturation point, is determined by the following formula:

$$\text{Per cent.} = \text{Pt./Atm.}$$

Where Pt. = vapor tension of the liquid at temperature t, and Atm. = pressure of the atmosphere. The vapor pressure (the force which a substance exerts to take the vapor form) of water and most common liquids is given in physical tables for all ordinary temperatures.

For example, at 20° C. (68° F.) water has a vapor tension of 17.363 millimeters of Hg. Air saturated with water vapor at 20° C., therefore, contains 2.28 per cent. If it only contains 1.14 per cent., its humidity is only 50 per cent. At 0° C. (32° F.) the vapor tension of water is 4.569 mm. and air saturated at 0° C., therefore, carries only .6 per cent. water vapor, or considerably less than if only 50 per cent. saturated at 20° C.

9. Strictly, air is not altered by increase in temperature so that it can carry more water vapor, but the liquid producing the vapor is changed so that it has a greater vapor tension and a proportionately greater percentage of vapor will enter the air.

10. The rate of evaporation then from any given moist surface depends upon the actual amount of water the air is capable of taking up (not on the *percentage* of unsaturation) and upon the relative rapidity of movement of the air.

For example, from a square centimeter of skin surface with saturated air no evaporation can take place at any temperature. At 20° C., however, and 50 per cent. humidity, let us say that 1.14 milligrams are vaporized per minute, then at 0° C. and 50 per cent. humidity only .3 milligram would be vaporized in the same time. Even absolutely dry air at 0° C. evaporates less moisture than at 20° C. and 70 per cent. saturated. This is on the assumption that the skin is equally moist at both temperatures and that there is equal movement or no movement of air in both cases.

The amount of moisture that may be in the air, whether greater or less, can have no physiological effect, for that already there can be considered a dry gas. There need only be considered whether or not conditions are such that much or little or none can be evaporated.

11. Perhaps the normal human organism requires the removal of the heat produced by mental or physical activity at a certain fairly definite rate. In such case both heat carried away by radiation and by evaporation must be considered, one in a measure supplementing the other. Low temperatures increase radiation and decrease heat loss by evaporation and *vice versa*.

For example, one extreme condition would be when the air is saturated with water vapor and at or above the body temperature. The body then has lost all temperature control. At any lower temperature there still remains radiation though the air be saturated.

The other extreme condition would be for extremely low temperatures where, though there is little cooling from evaporation and little moisture produced for evaporation, radiation is excessive and the only remedy is the prevention of radiation by additional clothing.

Fortunately at low temperatures when radiation is high, there is little loss from the latent heat of evaporation because air at low temperatures, even if perfectly dry, can carry away little water vapor. In addition to this balance of effect from outside influences, there is the most important physiological influence of perspiration increasing or decreasing the moisture to be evaporated.

Is it not possible, therefore, that the best

physiological condition is that where heat loss by radiation just balances that produced and there is little or no use for perspiration and its evaporation. In other words, is not the physiological process of perspiration exhausting and only necessary because the atmosphere is too hot to permit of removal of heat by radiation without the aid of positive physiological effort?

In Atwater's and Benedict's experiments the data given should show the amount of water eliminated to the air from the breath and through the skin. This represents a definite number of calories absorbed and should show under the conditions the relation of the amount of heat removed by evaporation to that removed by radiation.

These suggestions are made, as you can see, by a novice in hygiene but they may possibly throw a little side light on the problem.

LOUIS CLEVELAND JONES

UNIVERSITY FELLOWSHIPS

TO THE EDITOR OF SCIENCE: In SCIENCE of February 10, just at hand out here, there appears a letter by Dr. S. N. Patten upon which I would like the privilege of commenting.

The letter divides itself into two main contentions.

First, by means of a table of present occupations, an attempt is made to show the prosperity of a group of "fellows," thereby demonstrating the value of fellowships. To prove the point the investigation would have to be conducted along lines similar to those recently followed in another connection by Professors Furman, of Stevens Institute, and Cooley, of Michigan.[1] As it is, the surface indications presented by this table lead—through the legitimate inferences which may be drawn—to a conclusion diametrically opposite to the one there stated.

Of the 183 fellows listed as living and of known occupation, 27 belong to a group comprised of literary workers, social workers, ministers and students. To assume any of these wallowing in a wealth of financial returns offers difficulty to the imagination.

[1] *Proc. Soc. for the Prom. of Eng. Ed.*

The second group, 138, consists of 31 teachers in normal and secondary schools and 107 instructors and professors. Lacking specific knowledge as to these individual cases it is fair to reason that these men are no better off than the average of similar ages in their profession. From a knowledge of the prevailing conditions it would be proper to assume that $1,200 would not be far from the average salary of this group. Under present-day conditions this looks more like "starvation" to me than, perhaps, it does to one who pictures to himself living on an equal salary at the same age, under the conditions of thirty years ago.

This disposes of all save 18 experts and business men. Lacking specific evidence, it is fair to presume that this 10 per cent. is financially more prosperous than the other 90 per cent.

I hope that Professor Patten will complete his table, adding, for instance, the individual ages and salaries. This would give a chance for comparison with an equivalent group of non-fellows; at all events, it would transfer the matter from the realm of speculation to that of hard fact.

But it is the second of Dr. Patten's contentions which interests me more. He lays down the dictum that it is "rapidity of promotion and not lack of it that ruins promising investigators." To sustain this he gives a couple of inconclusive instances. In neither case are the returns, upon which to base final judgment, yet in.

To test his statement I jotted down a list of American scholars whose names occurred to me offhand, for one reason or another. Then I looked up their date of birth and of call to full professorship. The table follows, and the only name on my list omitted below is that of William James, whose call was deferred to his forty-third year because he started out in the field of anatomy and physiology and when he shifted to philosophy had his apprenticeship term as assistant professor to serve all over again.

Does this list bear out the conclusion that an early call or rapidity of promotion is the

ruin of promising scholars? I, for one, should like to see a much longer list like the foregoing compiled by, let us say, the editor of "American Men of Science." It would be interesting to see the result.

Name	Year of Birth	Year of Call	Age at Call
Gray, Asa	1810	1842	32
LeConte, Joseph	1823	1852	29
Gibbs, J. W.	1839	1871	32
Rowland, H. A.	1848	1876	28
Michelson, A. A.	1852	1883	31
Remsen, I.	1846	1872	26
Brooks, W. K.	1848	1876	28
Gildersleeve, B. L. ..	1831	1856	25
Hale, W. G.	1849	1880	31
Welch, W. H.	1850	1879	29
Osler, W.	1849	1874	25
Wilson, H. V.	1856	1885	29
Thurston, R. H.	1839	1871	32
Walker, F. A.	1840	1873	33
Fisher, I.	1867	1898	31
Sumner, W. G.	1840	1872	32
Giddings, F. H.	1855	1888	33
Clark, J. B.	1847	1877	30
Carver, T. N.	1865	1894	29
Seligman, E. R. A. ..	1861	1891	30
Ely, R.	1854	1881	27
Commons, J. R.	1862	1892	30
Patten, S. N.	1852	1888	36

If "young men should be left alone until they are fully developed before transplanting them" they form a curious biological exception. It is a novel contention that prolonged subordination best prepares for initiative and resourcefulness—for intellectual independence and leadership.

If "scholars are not born, they are made by their environment," some change in environment during their period of greatest growth might possibly prove to be stimulating and broadening—particularly if the change were such as, by offering easier financial conditions, freed their time from, let us say, assisting overworked wives in household duties.

The picture of President Jordan conducting a publicity bureau and "dragging into the limelight young men that it would have been better to leave alone" is enjoyable—their being in the statement just enough of that element of contrast which is the essence of humor.

It is true that President Jordan, at the opening of Stanford, called a group of professors whose ages ranged from 31 to 42 years, all but two of them, however, holding full professorships elsewhere at the time—but I, for one, am unaware of his having ever dragged any one of these, or his subsequent appointees, into the limelight. As for these subsequent appointees, their ages have been about 40, I should estimate, as an average. Stanford has offered no marked exception to the general trend of increasing age at promotion to full professorship.

In conclusion, I would like to say that it is significant to note the discussion called forth by the comparatively trivial question of fellowships, contrasted with the silence on the really vital subject presented in John Jay Chapman's letter to Science last summer. I wish you would reprint that communication now that the season of academic vacation is past.

Guido H. Marx

Stanford University, Cal.,
February 17, 1911

UNIVERSITY FELLOWSHIPS

To the Editor of Science: Reading Dr. Patten's criticism of Dr. Jordan's address I must protest against the whole tone of his argument, which seems to be that the inducements offered by the teaching profession are so promising and the chances of rapid promotion so great that they tend to prevent the production of scholars. I wish to give the view of a young man just facing the career of a teacher.

Dr. Patten's statement that "scholars are not born" is open to serious criticism. That they are "made by their environment" is partly true. I would extend this statement to read: "and also unmade by their environment," and this environment is a teaching position at $1,000 to $1,500 a year. He does not state how many of the 138 University of Pennsylvania fellows teaching in universities and secondary schools are struggling along on $1,500 or less trying to pay off debts. He does not state how many of them are unmarried because they can not afford it. I heard a

professor say that a young teacher should not get married, because the profession does not offer a living for two. Is this the price a man must pay for learning? Is it worth while? Does it lead to scholarship?

The assistant professor with a family can not, on $1,500 a year, get the necessary literature and books, he can not attend meetings at a distance, or travel and gather knowledge and inspiration from others of his kind. Does this lead to scholarship? Is it an inspiration for a university fellow to teach at $1,000 a year in a secondary school? Does this indicate that the market is not overstocked?

I raise a question as to whether "it is the professor that needs endowment" if we are to produce scholars. One reason there are not more scholars in America is because the entering inducements are not sufficient. Men prepare for the teaching profession and then turn aside for more remunerative work. In the zoological laboratory at Columbia University last year there were five students about ready to come up for the Ph.D. degree. Of these, three signified their intention of abandoning their plans of following the teaching profession. It is the young men who abandon the call of scholarship. They are the ones who need encouragement. It is the getting of more men into the profession that will produce scholars as well as helping those already in it. From the many will come the few real scholars that the nation can produce and these will be the result of heredity as well as of training.

It has been claimed that the greatest intellects in America are among the business men. This may be partly true. If the inducements of the teaching profession had been greater some of these men would, doubtless, be leading scholars to-day.

Dr. Patten's hopes of a promising man are that he will settle down on $1,500 a year, forgetting that he has a family to provide for, that he needs literature, travel and association with others. I can not forget seeing a university instructor spending his vacation wheeling a wheelbarrow. This is not going to produce scholars. It produces assistant professors without enthusiasm or inspiration. The teaching profession is bad enough. Why make it worse?

The idea that "no one should have the title of professor until it was fully earned" is good. But he should be supported while he is attempting to earn it. If this were done more generously than at present more men would make the attempt. Poverty does not offer a smooth road to learning.

C. V. BURKE

STANFORD UNIVERSITY

SCIENTIFIC BOOKS

Human Embryology—Keibel and Mall. Written by C. R. BARDEEN, Wisconsin; H. M. EVANS, Baltimore; W. FELIX, Zurich; C. GROSSER, Prague; F. KEIBEL, Freiburg, i. Br.; F. T. LEWIS, Boston; W. H. LEWIS, Baltimore; J. P. MCMURRICH, Toronto; F. P. MALL, Baltimore; C. S. MINOT, Boston; F. PINKUS, Berlin; F. R. SABIN, Baltimore; G. L. STREETER, Michigan; J. TANDLER, Vienna; E. ZUCKERKANDL, Vienna. Edited by FRANZ KEIBEL and FRANKLIN P. MALL. In two volumes. Volume I. 550 pp., with 423 illustrations. Philadelphia and London, J. B. Lippincott Company. 1910.

The publication of this work is worthy of very special notice, for it may well be said to mark an epoch of accomplishment in the study of human embryology, while on the other hand it furnishes exceptionally numerous suggestions of many problems yet to be solved, with the most promising lines of attack.

In the introduction, Professor Keibel brings out vividly, after an excellent historical review, the conditions which led up to the inauguration of the modern study of human embryology by Wm. His. It is fortunate that the great volume of these studies which have been accumulated under this inspiration should now be so fully reviewed and made available by the cooperation of these students of his. The plan of His for an extended exposition of human embryology is thus finally accomplished under the leadership of Keibel and Mall on two sides of the Atlantic.

Examination of the first volume gives quite a different conception of the field of human embryology from that obtained from other older text-books on the subject; for instance, Minot's very successful and constructive text-book of twenty years ago. It was evident then that, though much valuable work had already been done in various lines, it was still of a scattered character and unorganized. The effect, as a whole, was inconclusive. There were very few topics which had been studied with thoroughness or on sufficient human material.

In the new book we find that a considerable mass of fresh facts and accurate information have been accumulated, from intensive and systematic studies of a far more complete series of embryos, by more effective methods. Though much remains to be done in every department of investigation, it is now clear that the study of human embryos has become organized. Extensive collections are accessible. There is a systematic effort to obtain more complete records in every chapter of the history. The comparative and experimental method is not less used, but has become more an aid to interpretation; while the results secured from the study of human material itself now overshadow all else.

Another striking feature of the new book is the fact that it is the product of a cooperation between so many active American anatomists with well-known Germans; and that it is published, at the same time, in two languages. This is not only a recognition of the extent of His's influence, and of the general wide growth of a cooperative spirit in scientific anatomical research, which has become very marked in this science; it is also a substantial appreciation from abroad of the effective work which has been done in embryology among our anatomists in recent years. Almost every chapter of the book enforces this fact, and the use of illustrations and references to contributions from Americans published in the *American Journal of Anatomy* is frequent. Nine of the fifteen editors are Americans and, of these, five are pupils of Dr. Mall, whose initiative and stimulating example have encouraged much of the activity in embryological investigations of this character in this country.

Keibel writes the more general chapters in the first part (90 pages), including the historical introduction, the account of the germ-cells, fertilization, segmentation, young human ova, germ-layers and gastrulation, and a general summary of the development of the embryo. The rather restricted space given to these topics, which are treated so extensively in Hertwig's "Handbook of Comparative Embryology," is an illustration of the limitation of the scope of the new work to the results of the study of human material. Much of the greatest interest connected with these topics is here necessarily omitted, but there is a decided advantage in exhibiting the actual status of our knowledge. All through these chapters Keibel's fine critical judgment is evident. One receives the true impression that the writer has thorough personal familiarity with the facts. Nowhere is a nice judicial decision in such demand as in discussions about young human ova, hence we are especially glad to find this chapter by Keibel, whose treatment is masterful and illuminating. The tables are new and the details of comparison of young stages are satisfactory. The reader has an opportunity to secure a necessary amount of detail often lacking, confused or pointless in other accounts. The obstetrician will find much of interest here. Keibel's discussion of germ-layers is especially well put. The summary and résumé of the general history of the embryo based on the His and Keibel-Elze Nommentafeln of human embryos is excellent, the little critical accounts of each important embryo being well worth while.

Grosser's long chapter (90 pages) on menstruation, implantation, placentation, membranes, etc., is based on extensive personal investigations. This account is almost a monographic treatment of these subjects. The obstetrician will find a mine of discoveries in this chapter and the two following ones. The trophoblast, chorion, villi, decidua,

placenta, menstruation, ovulation, age of embryos and other related topics are given proper attention, and put in the most modern light. An excellent basis is laid by precise description for further work in the histological or pathological study of membranes and uterine conditions, and a better idea is now possible of the physiological environment of early embryos, especially from the new young human embryos described in detail.

In his chapters (60 pages), Mall advocates a new and improved method of measuring embryos, which may also be applied to the adult. His analysis of our knowledge of the age of embryos, growth rate, etc., is original, and results in new curves and tables of development. He thus furnishes us a standard, the result of a wide experience with all known data. His chapter on pathological embryos is novel and an advance in view point. It represents the essentials of his very extensive publications on this subject. The types of abnormalities, percentage of abortions, and the association of the pathology of the membranes with these are discussed. This leads up to an analysis of the bearing which experimental studies, mechanical and chemical, leading to the production of definite monstrosities in other forms, probably have on the origin of abnormalities in human embryos. Medical students and pathologists will turn to these chapters freely, and general embryologists will find here interesting significance in their work.

The second half of the first volume is devoted to the first chapters of the development of special systems of organs, which will be continued in the second volume. An examination makes it plain that these chapters were not written for beginners, though the treatment is direct and clear enough for any one. Even this first volume is sufficient to show that there is now ample material for satisfactory courses in mammalian embryology of advanced character, specially adapted to medical students.

It is evident that the time has come for our anatomical departments to insist that students beginning anatomy should have already obtained, during their two years of pre-medical college work now generally required, sufficiently thorough courses in elementary vertebrate embryology, to permit of properly introducing them to the later stages, in man and mammals, which have so much significance for their work in human anatomy.

The fifty pages by Pinkus, devoted to the integument, proves to be a consideration of many interesting aspects of the growth of the skin structures often not insisted upon. For in the treatment of pigmentation, glycogen, fat, glands, hair and hair tracts, and of the friction ridges and metamerism, we have emphasized the value of more general studies like those of Galton, Wilder, Sherrington, Bolk and Harrison. The special circulation, lymph supply, and nerve supply of the skin are largely left for the writers who shall describe these systems.

Bardeen's section (150 pages) on the skeleton and connective tissues may be characterized as encyclopædic in the inclusion of so great a mass of data. It is a storehouse of information, the variety of which has made the task of collecting and arranging very difficult. The author has exhibited much energy and skill in handling the material, to which he has contributed considerable of value. Mall's views are accepted as the most reasonable for the origin of the reticulum and fibrillæ of this tissue. Some of the most welcome sections on the skeleton are those on joints, and on variation and abnormalities in development. The origin and changes of form of vertebræ are illustrated by Bardeen's models, a set of which should be found in every anatomical laboratory. The treatment of the development of special regions of the skeleton, of ossification centers, of the length and curvature of the spine, is accompanied by valuable figures and diagrams. The skeleton of the limbs is taken up with similar fulness and contains further contributions from the author, many figures new to text-books, and useful tables. The skull is studied in forty pages with a proper reference to comparative embryology where also much has been done, and such important complicated regions as the

orbit, temporal and nasal regions, and the visceral arch derivatives are considered separately. The author publishes, for the first time, two new views of the skull of a twenty-millimeter embryo which combine numerous interesting relations. It is a pity that the details of these figures are not better shown in the half-tones. Anatomical illustrations are being steadily improved, and this is shown in many figures in this edition; but far more care is yet required in both the printing and selection of suitable paper, before the standard of German editions can be approached. The chapter closes with an extended account of the ossification of the individual bones of the skull.

The development of the muscular system is described by W. H. Lewis in about seventy pages. We find this an excellent account, well planned and comprehensive, while at the same time concise and logical. The influence of a participation on the part of the writer in embryological studies of an experimental nature is quite evident and gives a modern point of view.

The chapter is well illustrated, many of the figures being original, and some published here first. It is possible to obtain from them and the text a good idea of the origin and development of the various muscle groups. The author's contributions to the development of the muscles of trunk and limbs, and more recently of the head, tongue and larynx, are outlined here with figures from his new models. Futamura's striking series of pictures representing the stages of spreading of the facial musculature forms an interesting feature.

Only twenty-four pages are allotted to the last section, including the septum transversum diaphragm, and the cœlom, but the subject is brought up to-date by Mall with the aid of new investigations in addition to his previous extensive studies and the conditions found in a number of recently described young human ova. Broman's work is also incorporated to advantage.

One praiseworthy feature is the extensive bibliography at the end of each chapter.

Though the printing of the illustrations and the general make-up of the German edition is decidedly superior to the English, the American publishers have, on the whole, succeeded in making the volume a creditable one. The second volume, now in press, will be anticipated with much interest.

H. McE. Knower

Cincinnati, Ohio,
February 18, 1911

SPECIAL ARTICLES

THE ORIGIN OF NINE WING MUTATIONS IN DROSOPHILA[1]

In the following preliminary report I wish to put on record some of the principal wing mutations that have appeared in cultures of the fruit fly, *Drosophila ampelophila*. In another communication I shall describe five mutations in eye color that have been found in the same cultures, and their modes of inheritance. The theoretical questions involved must be deferred until the complete data can be published. These mutations have appeared in such rapid succession that my time has been almost entirely consumed in producing pure strains of the new forms, which can be utilized later for a thorough study of the inheritance of the new types. I wish here merely to call attention to the fact that while most of the new types breed true from the start, others do not; and also to the fact that while certain of the mutations are sex-limited other mutations involving the same organs do not show this form of inheritance. It may appear that we have here an opportunity to learn something further of these different modes of inheritance appearing in the same animal. One fact especially will impress itself on any one who follows the history of these new types, viz., the "segregation" of the characters, and in most cases the absence of intergrades.

Beaded Wings.—In May, 1910, a number of flies, pupæ, larvæ and eggs of *Drosophila* were subjected to radium rays. One fly was pro-

[1] The main facts in this paper were given before the American Society of Zoologists, December 29, 1910.

duced, the marginal vein of whose wings was beaded. Bred to his sisters, the beaded condition appeared in one fly in sixty of the next generation. The beaded flies were inbred and produced in the third generation, one beaded winged fly to thirty-five normal. The beaded flies produced in the next generation one beaded to twelve normal wings. The same process continued through many generations has finally produced stock that gives in certain cultures nearly 100 per cent. beaded wings.

Not only has selection slowly increased the percentage of abnormal wings, but much more extreme forms of the same variation have appeared, and an attempt is now being made to fix some of these extreme variations.

When a beaded male of the stock in its present condition was bred back to the wild females the beaded condition appeared in one fly in twelve of the first generation. In the second generation from non-beaded flies the proportion was one to twenty.

In the further course of the experiment eight other modifications have appeared that are undoubtedly mutations; one confined largely to females, some of the others showing strictly alternate inheritance, combined with "sex-limited transmission." These modifications may be described under the following headings.

Truncated Wings.—The normal wings of *Drosophila* are longer than the abdomen, extending for about one third their length beyond the end of the body. The wings have a rather pointed end. In the seventh generation of the beaded wing stock a fly appeared with the end of the wings cut off nearly squarely, and indented at the end or somewhat scalloped. Bred to sister flies there were produced in the next generation twenty-one flies with truncated wings to 230 with normal wings. In the next generation some of these truncated winged flies produced nearly 50 per cent. of flies with truncated wings. It has proved difficult to pass beyond this point, although certain of the later cultures have produced over 90 per cent. of truncated winged flies. This high average has not, however, been maintained in the next generation. There are indications, nevertheless, that the inheritance may be raised even to this high standard. Two main points of interest have appeared in the breeding of this modification. First, the condition is confined almost exclusively to the female line. A comparison of many cultures shows that the offspring of truncated parents consist of almost equal numbers of males and females with normal wings, while the truncated winged forms are, as stated, confined almost exclusively to the females. The deficiency of males in the entire culture is due, therefore, to the absence of males with truncated wings. Yet such males appear in small numbers, and especially in cultures in which truncated males are the fathers of the next generation.

Secondly, by selecting the shortest-winged individuals in each generation the wings have been reduced in some cases until they are not longer than the abdomen, and in a few cases they were even much shorter than the abdomen, and are not superficially different in this respect from the type to be next described, although in their inheritance they follow an entirely different procedure. The character can not be said to be sex-limited to the female in the conventional sense. When a female with truncated wings was bred to a normal wild male all of the offspring in the first generation had long wings. These inbred produced in the second generation 1,826 normal flies to 36 truncated individuals, males and females. When a truncated male was bred to a normal wild female all the flies in the first generation were normal, and in the second generation, when these were inbred, there were produced 1,408 normals to 14 truncated males and females. These facts indicate that this condition is not connected with sex-limited inheritance, although other wing modifications, as will be shown, are intimately connected with sex transmission. It should be added that normal winged flies of this modified stock throw a high, though variable, percentage of flies with truncated wings.

Rudimentary Wings.—In the second generation of the beaded winged flies a male ap-

peared with wings shorter than the abdomen. The square, short wings were broad and often blistered; in individuals obtained later their ends were crenated at times as though incompletely unfolded, but at other times the ends were quite even. This fly, bred to his sisters, reproduced the same condition in a few of his male offspring, and in closely related stock a few males of this kind appeared in successive generations. By repeated intercrossing a large number of these males have been produced and some females. It is important to note that although I have paired numbers of these males and females together I have obtained offspring in only a very few cases and these were all normal flies. Inbred these normal flies have produced 964 normal males and females; 6 short winged males and 2 females. Whether the original short winged females were virgins may appear doubtful from these facts. On the other hand the males and females with rudimentary wings are fertile, both with wild flies and with the mutations to be next described. The frequent failure to obtain fertile eggs from pairs of flies with rudimentary wings may seem therefore to indicate that this combination is generally sterile, but whether because the eggs are not fertilized or being fertilized do not develop is uncertain. This point is being further investigated. The extraordinary deficiency of individuals of this class in the second filial generation (see below), may be due to the same causes that account for the deficiency when inbred. Similar deficiencies, though not so extreme (see the second generation of white-red eyed and black-yellow crosses), run through nearly all of the results with my new races.

If a male with rudimentary wings is bred to normal females of *the original stock* all of the offspring have long wings. These inbred produce variable percentages of long and rudimentary wings. A partial census of the results gave 5,850 normal wings to 83 males with rudimentary wings. No intermediate types appeared. Later several males with rudimentary wings were bred to a new *wild stock*. The offspring had long wings. These flies inbred produced in the next generation 8,459 normals, males and females, and only 32 males with rudimentary wings. The experiments show that the condition of rudimentary wings is sex-limited. By suitable combinations this sex-limited character has been combined with another sex-limited character, viz., white eyes. The theoretical questions involved in such a combination have been discussed elsewhere.[2]

Miniature Wings.—In the seventh generation of the beaded wing stock a fly appeared with wings like the normal in form, but extending no further than the end of the abdomen. Flies with wings of this kind are much more viable than those with rudimentary wings. Pure cultures of thousands of individuals are now on hand. The character also shows itself to be sex-limited. Many combinations between these flies and those with other modifications have been made. Only a few of these can be now mentioned. Miniature wings have been combined with the white eyes, also a sex-limited character.

If a male with miniature wings is crossed with a wild female all of the offspring have long wings. These inbred have produced 409 flies with normal wings, males and females, and 178 *males* with miniature wings. No females with miniature wings appeared in the second generation. Miniature wings are, therefore, sex-limited in inheritance. The reciprocal cross, viz., females with miniature wings by wild males, gave in the first generation 50 females with normal wings and 51 males with miniature wings. Evidently the male-producing sperm of the wild fly does not carry the character essential for the formation of long wings. This same spermatozoon also lacks, as I have tried to show, one of the factors essential for the formation of red eyes, likewise a sex-limited character. The F_1 flies, inbred, produced 785 normal males and females (in approximately equal numbers) and 827 flies with miniature wings, of which 430 were females and 397 were males. This result gives the expectation for this combination.

[2] *Proc. Soc. Experimental Biology and Medicine*, Vol. 8, October, 1910, and *American Naturalist*, February, 1911.

It will be noted that the flies with miniature wings are even more numerous in this generation than the normal flies. Their chances for development are therefore at least equal.

Balloon Wings.—Occasionally flies have appeared, especially in the truncated winged stock, with each wing swollen up into a balloon or bladder filled with fluid. When these become dry the walls collapse and form flat plates that are carried at right angles to the body. Some of these flies have been isolated and have produced practically pure stock with the character present in all of the individuals, and this condition has been carried through two generations. The flies are exceedingly active and look like small X's running about in the bottles. They can not of course fly. They seem to be very sterile and produce only a few offspring, but nevertheless I have more than a thousand of these flies alive at present.

Albino Flies.—Several times flies have appeared that failed to develop black pigment in the body. The eyes were red, *i. e.,* unmodified by the lack of body pigment. One of the albinos had white eyes, it is true, but it arose in white-eyed stock, so that the colorless eye bore no relation to the absence of pigment in the body. The wings of these albinos were almost white with a faint brownish edge. The hairs of the body were brown instead of black. Although some of these flies lived for nearly a week they left no offspring. When the normal fly emerges it lacks pigment except in the eyes.

Melanistic Flies.—In some of the crosses between wild flies and those with miniature wings there have appeared in the second generation some flies, males and females, with black wings. The veins of the wings are broad and conspicuous. Pure stock of these flies was easily, and at once produced. The dark color is not confined to the wings, but the entire body is black. The type is clearly a melanistic variation of *Drosophila.* Crossed with wild stock the flies of the first generation are intermediate in color between the black and the normal type. The color does not seem to be sex-limited in relation to the normal, so far as the experiments have been carried.

Yellow Wings.—A male appeared in the black-winged stock with golden yellow wings. In fact, the entire fly is conspicuously yellow, and makes a striking contrast with his dark companions. He was bred to his black sisters, and gave only black flies in the first generation. These were inbred and have produced 233 black females, 127 black males and 76 golden-winged males. Evidently the color is sex-limited in relation to the melanic type from which it arose. The same, or a similar mutation, has appeared again in a stock not related to the first, except in so far as both came originally from flies with miniature wings. As yet only males have appeared. These have given in the first generation (when crossed to their normally colored sisters) normal flies.

Wingless Flies.—Occasionally flies appear without wings, but this character is not inherited, as a rule, and is due to some difficulty in unfolding the primordia of the wings. But in some of the stock of truncated wings I have obtained a considerable number of flies with tiny scales in place of wings. In one culture there appeared 11 flies with scales, instead of wings, amongst 125 winged flies. Although this stock is very sterile it seems not improbable that, in time, a wingless fly can be produced.

T. H. MORGAN

COLUMBIA UNIVERSITY

THE NATIONAL CONFERENCE COMMITTEE ON STANDARDS OF COLLEGES AND SECONDARY SCHOOLS

THE fifth annual meeting of the National Conference Committee on Standards of Colleges and Secondary Schools was held at the rooms of the Carnegie Foundation for the Advancement of Teaching, 576 Fifth Avenue, New York, N. Y., on Saturday, January 28, 1911.

All the organizations which send delegates to the conferences of this committee were represented, those in attendance being as follows:

President George E. MacLean, The State University of Iowa, representing the National Association of State Universities.

Headmaster Wilson Farrand, Newark Academy, representing the College Entrance Examination Board.

Professor Nathaniel F. Davis, Brown University, representing the New England College Entrance Certificate Board.

Dean Herman V. Ames, The University of Pennsylvania, representing the Association of Colleges and Preparatory Schools of the Middle States and Maryland.

Principal Frederick L. Bliss, The University School of Detroit, representing the North Central Association of Colleges and Secondary Schools.

Chancellor James H. Kirkland, Vanderbilt University, representing the Association of Colleges and Preparatory Schools of the Southern States.

President Henry S. Pritchett, representing the Carnegie Foundation for the Advancement of Teaching.

Dr. Kendric C. Babcock, as substitute for Dr. Elmer E. Brown, the United States Commissioner of Education.

Dean Frederick C. Ferry, Williams College, representing the New England Association of Colleges and Preparatory Schools.

In its desire to help in the establishment of a uniform and convenient terminology, the committee had requested a sub-committee to prepare for this meeting a report with regard to the use of the terms "hour," "count," "unit," "period," "exercise," "point," etc., in secondary schools and colleges. Mr. Farrand, chairman of the sub-committee, reported the results of the investigation which had been made and embodied these results in the following resolution, which was unanimously adopted:

Resolved, that this committee recommends, as a matter of convenience and to secure uniformity:

1. That the term *unit* be used only as a measure of work done in secondary schools, and that the term *period* be used to denote a recitation (or equivalent exercise) in a secondary school.

2. That the term *hour* be restricted to use in measuring college work, and that the term *exercise* be used to denote a recitation, lecture or laboratory period in a college.

3. That *unit* be used as defined by this committee, the Carnegie Foundation, and the College Entrance Examination Board, and that *hour* be used preferably in the sense of year-hour.

4. That the use of other terms such as *count, point, credit,* etc., in any of these senses be discontinued.

The sub-committee had been requested also to consider and report on the desirability of agreeing on the precise use of the terms "programs of study," "curriculum" and "course of study." Mr. Farrand reported that this question had been carefully considered by his committee in the light of the published resolutions of the National Association of State Universities and of the Association of American Universities and such further information as could be procured. While conscious of the desirability of uniformity in the use of these terms, the sub-committee felt that it was unwise to take any action until some common usage should have become established in many institutions.

Following a discussion of the question of a combination of the examination and the certification methods of admission to college, Dean Ferry presented the following resolution which was unanimously adopted:

Resolved, that this committee endorses the movement of various colleges in the direction of attaching weight to the school record of each candidate in connection with his entrance examination, and recommends to the colleges that such records be regularly used as an aid in determining the candidate's fitness for admission to college.

The question of the proper use of the term "honorable dismissal" was considered at some length and the secretary was requested to obtain further information on the subject and to report at the next meeting.

Various questions proposed for discussion were laid on the table and the sub-committee, consisting of Headmaster Farrand, chairman, Principal Bliss, President Pritchett and Dean Ferry, was continued with a request that it report again at the next meeting.

The officers of the past year were reelected as follows:

President—President George E. MacLean.

Vice-president—Headmaster Wilson Farrand.

Secretary-treasurer—Dean Frederick C. Ferry.

The full report of the proceedings of the conference will be printed for distribution to the members of the associations which are represented in the committee.

Frederick C. Ferry,
Secretary-treasurer

ANTHROPOLOGY AT THE PROVIDENCE MEETING

The annual meeting of the American Anthropological Association was held in Providence, R. I., December 28–30, 1910, in affiliation with the American Folk-Lore Society. The sessions were held in Manning Hall, Brown University. In the absence of President William H. Holmes, Professor Roland B. Dixon presided. The attendance was good and a number of important papers

were presented. On the morning of December 29 there was a joint meeting of the association and the Archæological Institute of America in Union Auditorium, at which Miss Alice C. Fletcher presided.

ADDRESSES AND PAPERS

In the absence of President Henry M. Belden, of the American Folk-Lore Society, his address was read by Dr. Charles Peabody. Some of the most important papers read at the joint meeting are represented in this report by abstracts. These are:

Recent Progress in the Study of South American Indian Languages: Professor ALEXANDER F. CHAMBERLAIN.

The author pointed out the regions of the South American continent to which, during the last five years, scientific research had been particularly active: the Colombia-Venezuela borderland, northwestern Brazil, Ecuador-Peru-Bolivia, southern Brazil, etc. Noteworthy are the investigations of Tavera-Acosta, Koch-Grünberg, Rivet and Beuchat, Farabee, E. Nordenskiöld, von Ihering, et al. To Tavera-Acosta we owe rather extensive vocabularies of the Guahiban, Piaroan, Puinavian, Salivan and Yaruran stocks, all of which hitherto have been rather scantily represented by linguistic material. Koch-Grünberg, as a result of his sojourns in northwestern Brazil, has shown the Makuan to be an independent stock, and added much to the linguistic material in print and in manuscripts concerning the Arawakan, Cariban, Betoyan, Miranhan and Uitotan stocks. Rivet and Beuchat, studying the extensive linguistic material obtained by the former of these authors (they are now working jointly), have thrown much light on the ethnologic problems of the Ecuador-Peruvian border-land, delimiting the areas of the Jivaran (Rivet has shown Brinton's "Jivaro" to be really Jebero and, therefore, Laman, or as he terms this stock, Cahuapana), Zaparan, Laman (Cahuapana), etc. Rivet believes that the Jivaran has marked Arawakan affinities, and his later studies claim to attach some of the minor stocks of southern Colombia to the Chibchan. Dr. Farabee's investigations have resulted in the accumulation of much lexical and grammatical material concerning the Arawakan peoples of Peru; also vocabularies, etc., from tribes of Pancan, Uitotan, Jivaran and other stocks. The thorough study of this valuable material will add not a little to our knowledge of the linguistics of the Peruvian area. E. Nordenskiöld has devoted some attention to the little-known tribes of eastern Bolivia, and we may expect other data of value from him in the near future. To von Ihering belongs the credit of having first established beyond a doubt the independent character of the Chavantean stock. Here should be mentioned also the researches of Barrett recently initiated into the language of the Cayapa, etc., of the Barbacoan stock. Of works of a more or less bibliographical character the most important are Lenz's monograph on the Indian elements in Chilean Spanish, Schuller's contributions to Araucanian bibliography, etc., and Mitre's "Catalogo," with its introduction by Torres.

Recent Literature on the South American "Amazons": Professor ALEXANDER F. CHAMBERLAIN.

The author résuméd and discussed the monographs of Lasch, Friederici and Rothery, all published during the year 1910. Of these the study of Friederici seems the most satisfactory; the book of Rothery, however, the most ambitious, treating the ancient and modern Amazons all over the globe. Dr. Friederici rejects the view of Ehrenreich and Lasch of a unitary origin of the Amazon legends among the northern Caribs, with extension thence over all northern South America. Both in content and origin the Amazon legends differ notably from each other in several cases, and they are of multiple provenance. In some there is evidence of modification and contamination through European sources. Among the causes of the origins of South American "Amazon" legends he enumerates the following:

1. The notably war-like character of women in many primitive American communities.

2. The peculiar power or influential position of women (due to economic, religious, hereditary or other social reasons) in a few tribes, which made a great impression upon the mass of the surrounding communities.

3. Rumors of the barbaric splendors of the empire of the Incas, which had penetrated the wilderness to the east.

4. Reports of certain unusual sexual relations of Indian women, etc.

5. Tales of "Amazons" due to native reports misunderstood by the Spaniards, or from such tales intentionally spread by the latter.

Amazon legends are reported from the West Indies (Ramon Pane records a characteristic one), from Yucatan and from Mexico. The Mexican legends, Dr. Friederici thinks, are "the least founded of all, ethnologically or mythologically"

(p. 23). Ill-founded likewise are the legends from California and the northwest Pacific coast. Incidentally, Friederici points out that the account attributed generally to Orellana, belongs really to Carvajal, and that the river of the "Amazons" received its name from the valor of the Indian women met with by the Spanish explorers.

The Uran: A New South American Linguistic Stock: Professor Alexander F. Chamberlain.

In 1891 Brinton recognized[1] in the region of Lake Titicaca a Puquina linguistic stock, observing at the same time that "the Puquinas are also known under the names of Urus or Uros, Hunos and Ochozomas." In this he was followed by R. de la Grasserie, in his "Langue Puquina" (1896), and others since then. In 1895–96, Dr. Max Uhle collected from the Urus of Iruitu, etc.,[2] a vocabulary of some 600 words, many sentences, etc., all of which material seems to be as yet unpublished. In 1897, J. T. Polo visited the Urus of Nazacara and obtained a vocabulary of some 350 words, 33 phrases, etc. This material did not appear in print till 1901[3] and constitutes the published linguistic data concerning the Uran stock. The author presented in English alphabetic arrangement a considerable portion of Polo's vocabulary, with grammatical notes, etc. Polo's work seems to have been practically unknown to ethnologists, Roman[4] being about the only one to recognize its importance and to see that the Uran and Puquinan must be unrelated. Careful examination by the author of this paper of the Uran linguistic material and comparison with Atacamenan, Puquinan, etc., prove beyond a doubt that the language of the Urus of Bolivia constitutes an independent family of speech. The few descendants of the ancient Urus, and the fewer still who keep their mother-tongue (many having adopted Aymará) are to be found scattered along the Rio Desaguadero between Lake Titicaca and Lake Aullagas or Poopo, particularly at Iruitu, Sojapata, Ancoaqui, Ahuallamaya, Nazacara, etc. In the past they evidently occupied a much wider area between these two lakes.

It is worth noting that the confusion of the Urus with the Puquinas began with Hervas, Garcilaso de la Vega, *e. g.*, among the older authorities distinctly separating the two languages. This separation of the Uran and Puquinan stocks clears up somewhat the linguistic difficulties of this region of South America, but leaves the Puquinas, their origin and the extent of their language-area, perhaps as much of a problem as ever.

[1] "American Race," p. 221.
[2] *Globus*, Vol. 69, 1896, p. 19.
[3] *Bol. de la Soc. Geogr. de Lima*, Vol. X.
[4] "Antiq. de la Rég. Andine," Vol. I., 1908.

The Age-societies of the Plains Indians: Dr. R. H. Lowie.

Age-societies have been ascribed by ethnologists to a large number of Plains tribes. A sharp definition of the age-factor results in limiting the number to the Blackfoot, Village tribes, Arapaho and Gros Ventre (Atsina). The question arises whether in these cases the age-factor is a basic or derivative feature. Investigation proves that the age-element is a subordinate feature, the collective purchase of ceremonial regalia, songs and dances being apparently the dominant motive.

Some Aspects of New Jersey Archeology: Dr. Charles Peabody.

Slides were shown illustrating the three celebrated strata at Trenton, N. J., on the glacial terrace above the Delaware River, viz., the black soil, the yellow loam, probably of immediate postglacial deposition and the true "Trenton" gravels underlying the yellow soil.

Attention was called to certain discoveries made during the season's work of 1910 by Mr. Ernest Volk, who has spent large portions of the last twenty-two years in exploration and observation of the region.

1. *The Bison Bone.* On June 22, 1910, in the sand pit of Mr. Ahrendt on the terrace was found an artificial pit; the cross section was: at the top six inches of black soil, under this one foot of yellow loam, and under this a red clay band one inch thick. In the pit were found the femur of a bison and accompanying it fine particles of charcoal. In the red band on one side of the pit lay a chipped water-worn pebble of argillite, and in the same red band to the left, a water-worn pebble of argillite, not chipped.

2. *The Artificial Pit.* On August 23, 1910, in the sand pit of Mr. Ahrendt, on the terrace was found another artificial pit; the cross section: at the top, six inches of black soil; under this yellow loam (with thin red bands) three feet six inches thick; and under this, overlying the pit, three or four inches of brown sand and charcoal. Nothing but charcoal of human provenance was found in the pit.

3. *The Natural Pit.* In the same sand pit, seven feet down under a somewhat similar series

of natural strata, was a pit made by ice, probably, or by some other natural agency. The importance of commenting on these three pits together consists in drawing attention to the similarity of geological and climatological conditions under which the pits were formed. Light is shed on the question of the contemporaneity of man with the post-glacial conditions which permitted the deposition of the yellow drift and the formation of the series of so-called "ice-pits"; the bison is added to the list of animals which lived as contemporaries with man at this epoch. A photograph taken by Mr. Volk was shown giving the negative in yellow loam of a large boulder which had fallen out; the similarity of forces which were sufficient to transport such boulders during the formation of the yellow drift with those forces undoubtedly of glacial origin that deposited great boulders in the gravels lower down was insisted upon. Reference was made to the continuity, accuracy and fidelity of Mr. Volk's work.

The Historical Value of the Books of Chilan Balam: Mr. SYLVANUS GRISWOLD MORLEY.

The recovery of aboriginal history in America is exceedingly difficult because of the absence of original sources from which it may be constructed. To this general condition, however, the Mayas of Yucatan offer a striking exception. Centuries before the Spanish conquest this intelligent people had developed an accurate chronology and a system of hieroglyphic writing by means of which they recorded their annals.

These aboriginal records were destroyed at the time of the Spanish conquest, but in the century that followed (1550–1650) there grew up a body of native writings called "The Books of Chilan Balam" in which were embodied much of the aboriginal history of this country. The case for and against these chronicles as reliable sources for the reconstruction of Maya history may be summed up as follows:

A. *Unfavorable*

1. Breaks in the sequence of the katuns, the unit of enumeration used in the chronicles for counting time.

2. Certain disagreements, usually of time, in the statement of facts.

B. *Favorable*

1. Very general agreement throughout.

2. Early date at which the chronicles were compiled (1550–1650), when the ancient history had not yet been forgotten.

3. Authorship by natives, many of whom had grown to manhood before the Spanish conquest, and who had had, therefore, opportunities for learning their ancient history at first hand, before European invasion and acculturation.

4. Many corroboratory passages in the early Spanish writers.

There are two important conditions, however, which will explain, in part at least, the discrepancies in the chronicles, which have been noted above under A.

1. The original manuscripts have never been studied and compared, and the present translation was made from hand copies only, a condition pregnant with possibilities for error.

2. The translation itself is not always accurate and indeed in several instances has been shown to be misleading and incorrect.

Recent Archeological Investigations in northern Guatemala: DR. ALFRED M. TOZZER.

The area occupied by the remains of the Maya civilization may be roughly divided into various provinces distinguished from each other by chronological considerations as well as by those dealing with assemblage, construction, the manner and method of decoration and others.

The area treated in detail is that which includes the northeastern part of the Department of Peten, Guatemala. Tikal is the first city of importance in this region. To the east is Nakum, first made known to the scientific world by Count de Périgny in 1908, and Naranjo, explored by Mr. Maler. In addition to these ancient sites the Peabody Museum Expedition of 1909–10 reports the new ruins to the north of Naranjo and Nakum of La Honradez, Porvenir, Azucar, Seibal 2d, and Holmul in Guatemala and those of Tšotšikitam in British Honduras.

These ruins are all characterized by the presence of one large court or plaza around which in most cases the greater number of stelæ and altars are placed. The plan in each case shows a system of oriented courts all connected with one another with very few detached buildings.

From a study of the dates now available it will be seen that this region occupies the first position in point of time in all the ruins of the Maya area. It is not possible to show at the present stage of the study of the archeology of this section that the Tikal territory was the center from which spread the influence responsible for the cultures of Copan and Palenque. From the evidently later character of certain of the stelæ at Tikal, it may be reasoned that not only was this region a center, which began very early in the

life of the Maya civilization; but that it continued to hold its important position until well toward the end of the time, when the southern Maya culture resigned its place of preeminence to that part of the Maya people living far to the northward.

The results of the investigations of the Peabody Museum Expedition of 1909–10 will appear in the *Memoirs* of the museum.

Cretan Anthropometry: Professor Charles H. Hawes.

Since Professor Boyd Dawkins and Dr. Duckworth concluded that the ancient Cretans belonged to the long-headed dark short Mediterranean race, the examination of additional ancient skulls and measurements of living Cretans made by me have gone far to confirm this conclusion, and to show that the *average* modern Cretan is a modification of this type and has a broader head than his ancestor.

Nevertheless, the facts here set forth demonstrate that the ancient Cretans or Minoans with their characteristic long head are still represented in the more inaccessible regions, and that the broadening element is due to the presence of brachycephals who are mainly confined to the plains and coasts. Further, the facts are interpreted to indicate that the broad heads are descendants of aliens, and in the main to prehistoric immigrants.

The data for Minoan skulls is obtained from 118 crania, of which I use here 78 male skulls (c. 2000 B.C.), leaving out those of the Late Minoan period, during which there is both archeological and anthropological evidence of an alien immigration. These 78 skulls yield an average cranial index of 74.0, and the long heads are to the broad heads as 5 to 1.

The data for modern Cretans is large, amounting to over 60,000 measurements and observations, and for this reason comparisons are at present confined to the cephalic index. Adding 199 Cretans measured by Dr. Duckworth to those measured in my expeditions of 1905 and 1909, we have a total of 3,183. But from these have been deducted foreigners, women and children and even Mussulman Cretans, leaving 2,290 modern Cretans as the basis for the following comparisons. These yield an average cephalic index of 79.0 to be compared with 76.0 (*i. e.*, 74.0, the cranial index, plus 2.0, allowance for the cephalic). The average modern Cretan is therefore mesaticephalic, midway between his ancestor, the ancient Cretan, and his neighbor, the modern Greek (c. 82.3); and the long heads are to the broad heads in the proportion of 5 to 4. The difference is appreciable and impels us to ask, do the descendants of the ancient Cretans, with a cephalic index of 76.0, exist in Crete to-day? If so, it is reasonable to suppose that the invading aliens have driven the natives up into the hills, and there we find them. Present in the plains, they predominate in the mountains. In the mountain plain of Lasithi (2,700 ft.) the average cephalic index is 76.5, with a proportion of 9 dolichocephals to 1 brachycephal. On the northern slopes of Mount Ida the cephalic index is 76.5. On the northern slopes of the White Mountains, in the west, in one village, 65 men averaged 76.9 compared with 79.9 in the plains immediately below. In the Messara Mountains of the center, the average was 76.9 in contrast to 80.9 in the plains. Twenty-eight skulls of revolutionists of 1821 and 1866 chosen at random from the mausoleum of a mountain monastery, yielded a *cranial* index of 74.2 and a ratio of $4\frac{3}{8}$ long-heads to 1 broad-head. In the less accessible mountain regions are thus to be found modern Cretans of similar cephalic index and ratio of dolichocephals to brachycephals to those of Minoan Crete.

How then has the *average* cephalic index risen in 4,000 years from 76 to 79? I have already suggested that this change is due to the presence of the descendants of prehistoric immigrants. Reviewing historic invasions, it is possible to dispense with both Turkish and Venetian somatological influence. Mussulmans have been rigidly excluded from these records and the Venetians, I have shown by a careful comparison of the Venetian-named Cretans with the rest, possess exactly the same average cephalic index, thus evincing a breeding-out in the course of nine generations of the infusion of Venetian blood that Crete received. This leaves us with the prehistoric invasions of the Achæans and the Dorians, which tradition, history and archeology attest. Anthropometry witnesses to an invasion of broad heads in the third Late Minoan period (1450–1200 B.C.). It is to the Dorian inroad, a migration of a people, rather than to the freebooting Achæans, that I attribute the chief part in the broadening of the Cretan head. This is best illustrated in the southwest corner of Crete in the eparchies of Sphakia and Selinon. The Sphakiots are by tradition and dialect Dorians, and seem to have maintained the purity of their blood by resisting all invaders and by the custom of endogamy. They and their neighbors have average cephalic

indices of 80.4 and 80.9 and the broad heads are in the majority of 3 to 2 and 3 to 1. If we assume, as many scholars do, that the Dorians ultimately came from Illyria we have an explanation to hand. The Illyric stock is unmistakable and exceptional in Europe to-day, in that it combines a broad head with a tall frame. In this southwest corner of Crete is a broad-headed people with a stature of 1,709 mm. (cf. Dalmatians 1,711 mm.) whereas the central and western Cretans average 40 mm. less.

A further test made with an instrument I had just invented, the comparison of the sagittal curve of the living head brings out a striking likeness between the brachycephalic Sphakiots, the Albanians (the oldest inhabitants of Illyria) and the Tsakonians, a tribe in the east of the Peloponnesus, 8,000 in number, who still speak a Dorian dialect unintelligible to the Greeks. These three peoples, all with claims to Dorian descent, separated by hundreds of miles, yield exactly similar sagittal curves, and their *normal* types very closely approximate, whereas the contrast to that of the Mediterranean race is extraordinary.

The Social Organization of the Winnebago Indians: Dr. PAUL RADIN.

The topics discussed by Dr. Radin included: (1) the village organization; (2) the phratries; (3) the clans (animal names, animal descent, clans); (5) the clan functions (clan feasts, clan names); (4) the ceremonials associated with the clans; (5) the clan functions (clan feasts, clan wakes); (6) marriage; (7) death and mortuary customs; (8) the hunt; (9) the warpath.

Dr. Radin closed with a general theoretical discussion of the phratries and clans.

The Religious Ideas of the Winnebago Indians: Dr. PAUL RADIN.

This subject was also treated topically: (1) the guardian spirits associated with the ceremonial societies and with the clans; (2) their "nature" (nature deities and "spirit" animals) and the specific powers they control; (3) the "inheritance" of guardian spirits *per se*, and in association with the clan and the ceremonial organization; (4) the "degrees" in the attitude toward guardian spirits; (5) the conception of life, death, future life and transmigration, its bearing on the social organization; (6) the ceremonial associated with the attainment of long life, with death, future life, transmigration, and miscellaneous religious beliefs; (7) the guardian spirits as the basis of the ceremonial organizations and the influence of their disappearance on the type of ceremonial organization; (8) the impossibility of separating the social and religious factors in their attitude toward the guardian spirits and the general conceptions; (9) discussion as to the probable historical development of the religious-social complex.

Polynesian Gods: Professor ROLAND B. DIXON.

The characteristics of the four great gods of Polynesia were discussed, and the relative importance of these deities in the different island groups pointed out. Kane, Ku and Lono were suggested as forming a connected group, with Kanaloa quite separate and differing in origin. It was suggested that the latter might probably be derived from a Melanesian deity, whereas the triad showed indications of an origin in Indonesia.

Polynesian and Melanesian Mythology: Professor ROLAND B. DIXON.

The myth incidents of the Polynesian and Melanesian areas were considered in their distribution, and in their relation to the mythology of Micronesia and Malaysia. The general results of this comparison seemed to accord with the theories of migration and cultural origins derived from a study of material culture.

A Pre-Pajaritan Culture in the Rio Grande Drainage: Dr. EDGAR L. HEWETT.

On the high bench lands bordering the Chama River on the south, the writer recorded in the summer of 1905 a large number of ruins of a different character from any of the well-known ancient Pueblo ruins of Pajarito plateau. During the past summer many more of the same character were noted and surveyed in the Ojo Caliente valley. These ruins consist of foundations of cobblestone inclosing rectangular rooms. Some of the ruin groups are of great extent. A typical group consists of a central circular structure of stone, probably in part subterranean, an open plaza surrounding it, then the foundation walls extending out in all directions. The entire settlement is divided into two parts by a narrow irregular street. That these ruins antedate the great community houses of the Pajaritan culture is shown by the facts that the walls are reduced to the grass level and that these ruins in some cases partly underlie the structures of the latter period.

Abstracts were furnished by some authors who were not able to be present and read their papers. These abstracts are also given:

A Note on the Persistence of some Mediterranean Types: Miss GEORGIANA G. KING.

In Italy and Spain one meets the local frescoes and portraits at times in the streets. I am told that Leonardos and Luinis abound in the Milanese, and a friend of mine has seen a mother and three daughters conspicuously Etruscan in Massa Marittima. Myself I have seen the following and can show photographs for the elder part (I have no modern photographs): In Siena, children like Matteo di Giovanni's. In Viterbo, a woman like the "Roman School." In the Emelia, women like Mantegna's and the local school. In Arles, women like the Roman Sarcophagi. In Venice, ecclesiastics like Gentile Bellini's; women like Carpaccio's. In Spain, women like the Lady of Elche.

These last are alike in the matter of figure and carriage and expression, as well as feature.

The Double Curve Motive in Eastern Algonkian Art: Dr. Frank G. Speck.

This paper presents a brief preliminary report of investigations in decorative art being carried on among the tribes of the northeastern Algonkian group, including the Abenadi, Penobscots, Passamaquoddies, Malisits, Micmacs, Montagnais and Nasdapis. The predominant design unit is a figure described for convenience as the "double curve," two opposing incurves. Variations of this elementary figure occur so universally throughout the region discussed, that the double curve motive is really characteristic. While it is also seen occasionally in Iroquois and Ojibway art, it is none the less distinctive of the northeastern Algonkians. Formerly the designs were produced in the moose hair and porcupine quill techniques, and by painting; nowadays most of the examples are seen in beadwork, except among the Nasdapi, where painted decorations still occur. In wood carving and etching on birch bark the more southerly tribes still preserve the old type of decoration.

The main body of material discussed in the paper is based upon collections made among the Penobscots, who are being made the subject of an independent monograph by the writer. Some forty typical forms of the double-curve design, showing different degrees of elaboration, are used. The simplest is the bare double curve, the modifications ranging up through highly complex examples with a score or so of compounded ornaments filling up the interior. In the more modified examples the original double-curve unit is sometimes hardly distinguishable on account of the numerous embellishments. Aside from simple ornament, not any particular symbolism has so far been found that would apply to the whole region. Investigations in the field of symbolism have only produced satisfactory results among the Penobscots, where the designs seem to have originally been floral representations with a magical medicinal value through the association of the design with the herbal remedies which play so important a part in the life of these Indians. Judging, however, from the lack of such an interpretation among the Malisits, so far as has been discovered, it would seem, at present, as though the matter will have to be investigated along independent lines in each particular tribal area.

Materia Medica of the Algonkian Indians of Virginia: Mr. J. Ogle Warfield.

This paper treats: (1) Of the subject as recorded by the early authorities. This is far from being full and concise and yet is of value even for the little information it contains. (2) The remnants of these tribes now remaining, having been so closely kept in contact with the English settlers and their descendants for the past three hundred years, have lost all ceremonial functions and ideas connected therewith; and have even lost the limiting of such practise to any particular person or coterie of such persons. That which they use is chiefly in the form of decoctions or "teas" made of barks and roots, which are gathered and made by the mother or grandmother of the family; outward applications are also used. Quite a number of such remedies were obtained. They are not simply recollections of the past, but are used and believed in firmly.

In the absence of Professor Hiram Bingham, his paper on "The Ruins of Choquequirau" was read by Mr. George P. Winship. It and the paper (read by title) by Mr. Stansbury Hagar on "The Four Seasons of the Mexican Ritual of Infancy," are to be published in the *American Anthropologist*. Dr. Edward Sapir's two papers, "The Wolf Ceremonial of the Nootkas" and "The Linguistic Relationship of Nootka and Kwakiutl," will also appear in the *American Anthropologist*.

The papers read, of which the secretary was unable to obtain abstracts were:

Professor Wm. H. Goodyear: "Measurements in 1910 in the Spiral Stairway of the Leaning Tower of Pisa."

Dr. Elihu Grant: "Philistine and Hebrew in Palestine."

Professor E. M. Fogel: "The Survivals of Germanic Heathendom in Pennsylvania German Superstitions."

Professor Arthur C. L. Brown: "Fire and Fairies with Reference to Chrétien's Yvain, vv. 4385–4575."

Mr. Phillips Barry: "A Garland of Ballads."

The following papers were read by title:

Professor William H. Holmes: "The Place of the Esthetic in Human Welfare."

Professor Junius Henderson: "Tewa Ethnozoology."

Mr. W. W. Robbins: "Tewa Ethnobotany."

Miss Barbara Freire-Marreco: "Notes on Tewa Medical Practise."

Mr. John P. Harrington: "The Mesquite and its Uses."

Dr. Walter Hough: "The Dog in Pueblo, Mexican and Peruvian Mortuary Customs."

Miss H. Newell Wardle: "The Cradle-board in Ancient Mexico."

At one o'clock on Wednesday, the twenty-eighth, the corporation of Brown University gave a luncheon in the Administration Building, President Faunce receiving. The afternoon of the same day was devoted to sight-seeing; visits were made to the John Hay Memorial Library, the John Carter Brown Library, the Ann Mary Brown Memorial and the Rhode Island School of Design, followed by a reception at the Providence Art Club.

GEORGE GRANT MACCURDY
YALE UNIVERSITY,
NEW HAVEN, CONN.

FIFTH ANNUAL MEETING OF THE ENTOMOLOGICAL SOCIETY OF AMERICA

THE fifth annual meeting of the Entomological Society of America was held at the University of Minnesota, Minneapolis, December 27 and 28, in the school of mines building. The president, Dr. J. B. Smith, presided throughout the session. In the absence of the secretary-treasurer, Professor J. G. Sanders was elected secretary pro tem.

The following papers were read during the session:

"Notes on the Tingid *Leptobyrsa explanata* Heid.," E. L. Dickerson.

"Notes on *Sanninoidea exitiosa*," J. B. Smith

"The Structure of Spermatophores in Crickets," J. P. Jensen.

"The Biological Survey of the Insect Life of Kansas," S. J. Hunter.

"An Experimental Study of the Death-feigning Habits of *Belostoma* (*Zaitha*) *flumineum* and *Nepa apiculata* Uhler," H. C. and H. H. Severin.

"Announcement of Further Results secured in the Study of Tachinidæ and Allies," C. H. T. Townsend.

"Some Suggested Rules to govern Entomological Publications," T. D. A. Cockerell.

The report of the committee on nomenclature was received and ordered printed.

The report of the executive committee showed that nineteen new members had been received during the year and four lost through death.

The result of the mail vote ordered by the society at the Boston meeting was that the annual dues of the society should be two dollars, this to include a subscription to the *Annals of the Entomological Society of America*.

The following officers were elected:

President—Professor Herbert Osborn.

First Vice-president—Professor Lawrence Bruner.

Second Vice-president—Professor Alex. D. MacGillivray.

Secretary-Treasurer—Professor Alex. D. MacGillivray.

Additional Members of the Executive Committee—Professor J. H. Comstock, Professor J. B. Smith, Professor C. J. S. Bethune, Dr. W. M. Wheeler, Dr. H. Skinner, Dr. A. D. Hopkins.

The annual public address was given in Handicraft Hall by Professor F. L. Washburn, "The Typhoid Fly in the Minnesota Iron Range."

ALEX. D. MACGILLIVRAY,
Secretary-Treasurer

SOCIETIES AND ACADEMIES

THE NEW YORK ACADEMY OF SCIENCES
SECTION OF BIOLOGY

A REGULAR meeting of the Section of Biology was held at the American Museum of Natural History, January 16, 1911, Chairman Frederic A. Lucas presiding. The following papers were read:

Cryptomeric Inheritance in Onagra: C. STUART GAGER.

An abstract of this paper appeared in SCIENCE for February 3, 1911, p. 191.

Field Notes on Japanese Whales: ROY C. ANDREWS.

The speaker gave an account of a recent seven-months' stay at the Japanese whaling stations, telling of the methods employed in capturing and preparing the whales for commercial use; also of new notes on the habits of finback, blue and sei whales.

The latter species, called by the Japanese "sar-

dine whale,'' is referable to *Balænoptera arctica* Schlegel, and although it has been taken for a number of years at the Japanese stations almost no material relating to it is extant. The species is so closely allied to *Balænoptera borealis* Lesson of the Atlantic that further investigation will probably prove it synonymous.

Photographs of the rare North Pacific blackfish (*Globicephalus scammoni*) and of several species of dolphins were also shown. It was announced that a new porpoise of a most peculiar body shape had been secured, and would be described in a future number of the *American Museum Bulletin*.

At the regular meeting of the section held at the American Museum of Natural History, February 13, 1911, Chairman Frederic A. Lucas presiding, the following papers were read:

Climate and Evolution: W. D. Matthew.

The thesis of the paper is as follows:

1. Secular climatic change has been an important factor in the evolution of land vertebrates and the principal known cause of their present distribution.

2. The principal lines of migration in later geological epochs have been radial from holarctic centers of dispersal.

3. The geographic changes required to explain the past and present distribution of land vertebrates are not extensive and do not affect the permanence of the ocean basins as defined by the continental shelf.

4. The theory of alternations of moist and uniform with arid and zonal climates associated, respectively, with partial submergence and extreme emergence of the continental areas, as elaborated by Chamberlin, are in exact accord with the apparent course of evolution of land vertebrates, when interpreted with due allowance for the probable gaps in the geologic record.

5. The numerous hypothetic land bridges in temperate, tropical and southern regions, connecting continents now separated by deep oceans, which have been advocated by various authors, are improbable, inconsistent and unnecessary to explain geographic distribution. On the contrary, the known facts point distinctly to the permanency of the deep-ocean basins during the later epochs of geologic time, to the alternate connection and separation of the land areas within the line of the continental shelf and to the continued isolation of those land areas which are surrounded by deep ocean.

These theories are substantially an adaptation of the conservative views of Wallace and other zoologists to the geological theories of Chamberlin. They are defended by a consideration (1) of the nature and extent of the defects in the geological record; (2) of the relations of the zoological regions to each other and the changes effected by elevation or submergence of 100 fathoms; (3) of the principles of dispersal of land animals; (4) of the character of the fauna of oceanic islands (including Madagascar, Cuba and New Zealand) and the degree of probability which attaches to accidental transportation as a means of populating them; (5) of the present and known past distribution of the mammalia, group by group, in considerable detail; (6) of the distribution of the different orders of reptilia in a less detailed manner; (7) of the distribution of birds and fishes, with a few instances from invertebrate distribution which have been especially urged in support of hypothetical bridges; (8) of the objections to such bridges and an interpretation of the real significance of such evidence as has been adduced in support of them.

The speaker believed that the supposed cumulative evidence obtained in various groups of animals or plants for various continental bridges is due simply to identical errors in interpretation running through all such instances. On the other hand, to admit such bridges would seem to involve certain distribution results, which, in the groups which he has studied, assuredly do not exist.

The Limbs of Eryops and the Origin of Paired Limbs from Fins: W. K. Gregory.

In a skeleton of the temnospondylous amphibian, *Eryops megacephalus* Cope, from the Permian of Texas, which is now being mounted in the American Museum, the limbs are of special interest. Many resemblances to the contemporary reptile *Diadectes* are seen: in the stout, long coracoscapula, the short, wide-headed humerus, with its very wide, prominent and backwardly directed entocondyle, in the short fore-arm, in the very heavy, solid pelvis, stout femur and fully ossified carpus and tarsus. In the character of its limbs *Eryops* was on the whole nearer to *Sphenodon* than to the Urodeles, though far more archaic than the former. As shown by the facets, the humerus and femur were held almost at right angles to the body, the opposite feet being held very widely apart.

The generalized character of the limbs of *Eryops* with respect to those of higher Tetrapoda invite renewed inquiry into the origin of paired limbs from fins. The limbs of known branchio-

saurs and microsaurs do not carry us very far back toward any known type of fish fin. In these orders the cylindrical shafts of the long bones, with cartilaginous ends, the cartilaginous carpus and tarsus, the weak shoulder girdle and pelvis suggest a secondary adaptation to aquatic habits.

From the work of Thacher, Goodrich, Dean, R. C. Osburn and others, it seems probable that the paired fins of fishes, like the median fins, have evolved from wide-based fins with serially arranged basal and radial cartilages. After the formation of the primary shoulder girdle and pelvis and of the pro-, meso- and metapterygia by fusion and growth of the basals, the various types of paired fins seen in plagiostomes, chimæroids, pleuracanths, dipnoans, crossopterygians and actinopterygians seem to have arisen in each case through the protrusion of the basal cartilages, differential growth and shifting of the radials, and in some cases (*e. g.*, pleuracanths, crossopterygians, dipnoans) also through the extension of the radials around to the post-axial side of the metapterygial axis. If the Amphibia have descended from forerunners of teleostomous and dipnoan fishes (as seemed likely) then it was entirely probable that their paired fins had been transformed into limbs through the extreme protrusion of the proximal cartilages, differential growth and regrouping of the more distal cartilages, reduction of the dermal rays. This structural change may well have been in large part effected before the air-breathing proto-amphibians had left the water, owing to the assumption of a new function in the paired fins, *i. e.*, pushing against solid objects such as roots, in the stagnant water, instead of merely steering. A study of the pectoral girdle and fins of *Sauripteris*, a rhizodont crossopterygian of the Upper Devonian, in comparison with those of *Polypterus* and with the limbs of primitive amphibians, had suggested the following provisional homologies:

Crossopterygian	*Tetrapod*
"Infraclavicle"	= Clavicle
"Clavicle" (of Parker)	= Scapula
"Supraclavicle"	= Cleithrum
"Coracoid" (hypocoracoid)	= Coracoid (or precoracoid?)
"Scapula" (hypercoracoid)	= Humerus
Proximal basals	= Radius and ulna
Distal basals	= Carpals
Radials	= Metacarpals and phalanges
Dermal rays (derived from scales)	= Nails, scales

The reduction and loss of the post-temporal may have accompanied the freeing of the shoulder girdle from the skull. These views differ radically from those of Owen, Parker and Gegenbaur. The paired fins of *Sauripteris* were the only ones known that seemed to foreshadow even in a remote degree the paired limbs of the Tetrapoda. In the pelvic fin of *Eusthenopteron*, another crossopterygian of the Upper Devonian, differential evolution of the basals and radials had brought about certain remote resemblances to the tetrapod limb. The ilium of tetrapods appeared to be a neomorph.

L. HUSSAKOF,
Secretary

THE AMERICAN CHEMICAL SOCIETY
NEW YORK SECTION

THE sixth regular meeting of the session of 1910–11 was held at the Chemists' Club on March 10, Professor Chas. Baskerville in the chair.

The election of officers of the section for the session of 1911–12 resulted as follows:

Chairman—A. C. Langmuir.

Vice-chairman—A. B. Lamb.

Secretary and Treasurer—C. M. Joyce.

Executive Committee—Chas. Baskerville, David Wesson, W. J. Evans and Rudolph Seldner.

The following papers were presented:

"The Sulphides of Iron," E. T. Allen.

"The Detection of Salicylic Acid," H. C. Sherman and A. Gross.

"A Study of the Factors involved in the Qualitative Determination of Barium," E. Frankel and L. J. Curtman.

"The Detection of the Platinum Metals by the Glow Reaction," P. Rothberg and L. J. Curtman.

"Some New Rare Earth Compounds," Charles James.

"Experimental Furnaces for Glass Melts," P. C. McIlhiney.

"Examination of Commercial Oxygen," Reston Stevenson and Chas. Baskerville.

Before the reading of the last paper, the chairman asked Dr. A. H. Sabin, the first chairman of the section, to preside. Dr. Sabin, in taking the chair, gave a short account of the early history of the section.

Dr. McMurtrie had hoped to be present to make some valedictory remarks on the occasion of the last meeting in the original quarters of the Chemists' Club but was obliged to send a written com-

munication instead, which was read by Professor Baskerville.

C. M. Joyce,
Secretary

THE HELMINTHOLOGICAL SOCIETY OF WASHINGTON

The third regular meeting of the society was held in the zoological division of the Public Health and Marine Hospital Service on January 6, 1911, Dr. Stiles acting as host and Mr. Crawley as chairman.

Dr. Pfender presented a paper entitled "A Few Brief Remarks on Medico-zoological Nomenclature, with Special Emphasis on the Correct Terminology of the Protozoon of Syphilis." The paper points out that persons engaged in preparing new material are in duty bound to use all means that might aid in a clear and correct presentation of their subject. In the medico-zoological field, failure to appreciate this fact has resulted in burdening the literature with numerous erroneous names the errors being due to medical men and zoologists alike.

In the case of the organism of syphilis, the original errors were made by zoologists, but since being corrected have been persisted in by medical men and other scientific men. Schaudinn in 1905 reported the discovery of an organism which he believed to be the cause of syphilis. Believing that this belonged in the same genus as *Spirochæta plicatilis* Ehrenberg, 1834, he proposed for it the name *Spirochæte pallida,* adopting the emended spelling of Cohn, 1872, for the generic name. About this time Stiles called attention to the fact that Cohn had emended the spelling without reason or authority, hence the correct name was *Spirochæta.* The same year, 1905, Vuillemin decided that the organism did not belong in the genus *Spirochæta* and proposed the name *Spironema,* a change which was accepted by Schaudinn. Vuillemin, however, had not used due care in selecting his generic name, which was preoccupied by Meek, in 1864, for a genus of molluscs, and also, erroneously, by Klebs, in 1892, for a genus of flagellates. Being apprised of this fact, Schaudinn proposed the generic name *Treponema.* This name did not come to the attention of Stiles and Pfender in time to prevent their proposing the generic name *Microspironema,* this last name becoming a synonym under the law of priority. The correct name of the organism of syphilis is therefore *Treponema pallidum* Schaudinn, 1905, and not the popular *Spirochæta pallida.*

The conclusions to be drawn from this case are: (1) it is mandatory that the proper classification of an organism be determined before naming it; (2) every effort should be made to ascertain whether a contemplated name is preoccupied before publishing it; (3) when errors of nomenclature have been made, it is the duty of medical men and other scientists to avoid their repetition, regardless of the popularity of the names; (4) the difficulty of complying with these conditions and with the rules of nomenclature indicates the need of a reform, and this may perhaps be found in the selection of a tribunal of authorities to which proposed names must be submitted for approval and by which the status of names may be settled.

Mr. Hall presented two short notes on gid. The first one, accompanied by the exhibition of a couple of skulls of sheep dying of gid, called attention to the fact that the skulls of yearling sheep and lambs are very thin, and that in the case of sheep having gid, this thinness is increased in the vicinity of the parasite, commonly resulting in actual perforation of the skull in a number of places. The natural thinness and the thinness and perforation resulting from gid make it easy for dogs to get at the parasite and become infected. Experiment shows that they do this either by crushing and eating practically the entire skull, or by inserting the tongue in the foramen magnum and licking out the brain.

The second note dealt with a case of unusual delay in the development of the adult tapeworm, *Multiceps multiceps,* in the dog after feeding the larval gid bladderworm from the brain of the sheep. In this case the dog was killed 82 days after the feeding, and examined post mortem, the feces up to this time having shown no tapeworm eggs or proglottids. Five specimens of the gid tapeworm were found in the dog, the largest having eggs in the uterus, but with no onchospheres as yet developed. The two smaller specimens were less than half an inch long. The dog had been on a diet limited to salt meats or cooked beef and bread, and had had no opportunity to acquire infection with any tapeworm except *Dipylidium.* The usual period of development of the gid tapeworm is given by various authorities as from four to eight weeks. The experiment indicates the unreliability of conclusions based on negative fecal examinations and statements as to the usual time required for the development of a parasite.

Dr. Stiles presented a paper entitled "The Influence of Hookworm Disease upon the Apparent Age of Children in the Cotton Mills." There is a wide-spread popular belief that many very young children are working in the mills, and that this life results in retarding their physical development and in producing what is called a typical "cotton-mill child." Regarding this point, it appears from numerous observations that among the rural tenant whites, children very commonly appear to be younger than they are. This is frequently due to hookworm disease retarding development. A study of the real and apparent age of 42 people, part of whom were employed in the mills and part not so employed, showed that these persons appeared on an average 3 to 3.8 years younger than they actually were. These cases show that the stunted growth of the "cotton-mill child" is found in persons who never worked in cotton mills and in those who had never even lived in a cotton-mill village. All these cases were marked hookworm cases and their stunted growth was apparently due to this disease. Considering these facts with other data, the conclusion to be drawn is that the "cotton-mill child" like "cotton-mill anemia" is a product, not of the cotton mills, but of soil pollution and of the resulting infection taking place before puberty. Owing to the presence of hookworm suspects, great care is necessary in judging the ages of mill children.

Even after making allowance for hookworm cases in judging ages, there are still a number of young children at work in the mills. These are in some cases actually below the legal mill-age limit. Of 464 hookworm cases and suspects where the exact or approximate age was obtained, 19, over 4 per cent., were below the legal mill-age limit (twelve years) of South Carolina, the ages running from nine to eleven years. Probably the ratio of children under twelve years of age to the total number of employees is between 2 and 5 per cent.

A number of the small children seen in the mills are not employed by the mills, but are "helping" their elders. The parent or elder sister is paid for the amount of work turned out. With the assistance of a younger brother or sister, who would otherwise be helping with the housework at home, more work can be turned out, thus increasing the earnings. The employees are chiefly responsible for this form of evading the law. Most mills would prefer not to have these "helpers," but if they are turned out, the family involved moves to another mill. The practise can only be stopped by legal prohibition or mill-association agreement, the former being perhaps the better, as the employees resent restrictions made by the mills but accept in more or less good faith the restrictive laws.

Mothers often bring children four or five years old to the mills and the children play while the mothers work. Often the mother can not leave the child at home, and the kindergarten, provided by some of the mills, is not always open. To refuse to allow these children in the mills would increase the difficulty of a widowed or deserted mother in supporting herself.

The average child in the cotton-mill family is anxious to go into the mills. To this end many of them practise deception in regard to their ages.

Cases probably occur where parents compel children to lie about their ages; the large number of children claiming to be just above the legal age for mill work points to this.

Many of the children have no idea of their exact age and the fathers are often almost as ignorant.

It seems probable that a physical examination by local or state medical officials would be better than the present age limit laws. In this way many cases of disease would be detected, excluded from the mills and treated. It would exclude from the mills all persons who are now justly excluded.

Dr. Stiles presented a second paper entitled "Is the So-called 'Cotton-mill Anemia' of the Gulf-Atlantic States due to Lint or to Hookworms?" A common explanation of the wide-spread anemia among cotton mill hands in certain portions of the United States is that it is due to "breathing in the lint." If this be true, then since the cotton, and hence the lint also, are practically identical in all parts of the country, we would expect to find: (1) that the hands in the New England cotton mills show the same type and extent of anemia as those in mills south of the Potomac and Ohio Rivers; (2) that mills of a similar character in eastern, central or western parts of North or South Carolina, or in southern, central or northern parts of Georgia or Alabama, show approximately the same proportion of anemia; (3) that there is the same amount and severity of anemia in a given district among hands who are of the first, second or third generation of mill workers and removed by one, two or three generations from the farm, or if there were

any difference that the anemia would be less among the hands of the first generation; (4) that the greater proportion of severe cases of anemia will be found in such rooms as the card room where there is the greater amount of lint flying around, less severe cases in the spinning room, and no severe cases in the cloth or engine room where there is little or no lint; (5) that persons fresh from the farms will not be very anemic but will become more so the longer they work; (6) and that if the anemia is due to the lint it is not very extensive or severe in the knitting mills, the paper mills, or the orphan asylums, or among medical students or trained nurses, since the persons involved here have little or no contact with lint. Investigation shows that in a general way the contrary is true in the above assumptions.

(1) Not a single case of cotton-mill anemia was found in the New England mills, although over 2,000 had been seen in the southern mills. (2) In the Carolinas, Georgia and Alabama, there is a relatively low percentage of cases of anemia in the central clay lands, a greater number of cases and more severe cases are found in places where the hands come from the sand or mountain lands, the worst cases coming from the sand lands. (3) Even where they were located in new mills with better sanitary provisions, the generation fresh from the farms showed a greater percentage of anemia than the hands who were one or two generations removed from the farm. (4) In general the cases of cotton-mill anemia are more severe and frequent in the spinning room than in the card room, and the most severe case found in one mill was in the cloth room. (5) Of 59 cases, 39 per cent. had been in the mills less than a year and 61 per cent. had been there more than a year. It seems that 94 per cent. of these cases might theoretically be considered as hookworm infections persisting after removal from soil pollution, and in a number of cases of severe anemia the persons had just entered the mill. (6) The most anemia found in any establishment was in a knitting mill, where 68.8 per cent. had typical "cotton-mill anemia." This same anemia was found in paper mills and colleges, and 18 cases were found in an orphanage among children who had never worked in the mills.

The above findings lead to the conclusion that the "cotton mill anemia" has nothing to do with working in the cotton mills. It is due to hookworm disease, contracted in most cases on the farms as a result of the insanitary conditions existing in the rural districts, and brought to the mills from the farms. There is less anemia among mill workers than among the farm tenants, owing to the fact that life under the improved sanitary conditions of the average cotton mill is better from the public-health point of view than life as a tenant on a soil-polluted farm of the sort from which so many of these people come.

Mr. Crawley presented a paper entitled "Experiments on the Transmission of Trypanosomes by *Glossina*," in which he reviewed the work of Bouet and Roubaud. These workers instituted a series of experiments with *Trypanosoma dimorphon*, *T. cazalboui* and *T. pecaudi*, and the tsetse flies, *Glossina palpalis*, *G. tachinoïdes* and *G. longipalpis*. It was found that any one of the three species of *Glossina* functioned equally well as host for *T. cazalboui*. Observations with the other trypanosomes have not been completed. Flies bred in the laboratory, and hence free from parasites, were permitted to feed on an infected animal and later were fed each day on a series of "clean" animals. It was found that the first feed or "infecting repast" was followed by a latent period of six days, during which the trypanosomes in the fly are innocuous. The fly then becomes infective and for some time at least the number of trypanosomes within it undergoes a progressive increase. The fly presumably remains infective as long as it lives.

An examination of the infected flies showed that the infection was confined to the proboscis. In the labrum, the flagellates are of the *Leptomonas* type and are fixed to the wall. These develop into trypanosomes which occur in the interior of the hypopharynx. They are either attached to the walls by the ends of the flagella, or free and motile in the interior of the organ. They never ascend beyond the common canal of the salivary glands, but always amass in considerable numbers in the vicinity of the opening of this canal into the hypopharynx.

These trypanosome forms appear in the hypopharynx forty-eight hours after the infecting feed, and are abundant by the fourth day. The authors regard these as the infecting elements, and call attention to the fact that the incubation period is not marked by morphological changes, but rather by changes of a physiological nature as regards the virulence of the parasites.

In all, 20 per cent. of the flies fed on infected animals became infected.

Maurice C. Hall,
Secretary